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# PHILOSOPHICAL
# TRANSACTIONS,

OF THE

# ROYAL SOCIETY

OF

# LONDON.

FOR THE YEAR MDCCXCIX.

PART I.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCIX.

BRITISH MUSEUM NATURAL HISTORY

# ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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*Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.*

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1798, two Medals on Sir GODFREY COPLEY's Donation; one to Sir GEORGE SHUCKBURGH EVELYN, Bart. F. R. S. for his various Communications, printed in the Philosophical Transactions; the other to CHARLES HATCHETT, Esq. F. R. S. for his Chemical Communications, also printed in the Philosophical Transactions.

# PHILOSOPHICAL

# TRANSACTIONS.

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## I. *The Croonian Lecture. Experiments and Observations upon the Structure of Nerves. By* Everard Home, *Esq. F. R. S.*

Read November 8, 1798.

Having had the honour of laying before this learned Society several Lectures on the actions of different parts of the organ of vision, the prosecution of the same inquiry has led to some observations on the internal structure of the optic nerve, which will be explained in the present Lecture.

On the first view, the structure of nerves may appear an improper subject; but, when their offices and connection with muscles are maturely considered, any knowledge respecting them will be allowed an important acquisition towards the investigation of muscular motion.

In bringing forward an account of newly-acquired facts, the most natural, and therefore the most satisfactory method is, to begin with the circumstances which led to their detection. This at present becomes the more proper, as the experiments which brought the subject of nervous structure under consideration, were made upon the eye, and were in some measure connected with the observations contained in the former Lectures: they were instituted with a view to ascertain the cause of the luminous appearance frequently observed in the cat's eye.

The illumination so conspicuous in the eye of the cat, and of many other animals, when seen in an obscure light, has attracted the attention of every common observer. Philosophers also have paid particular attention to it, and have endeavoured to investigate the cause. On this subject there have been two opinions: one, that the illumination arises from the external light collected in the eye, and reflected; the other, that there is a quantity of light generated in the organ itself.

Professor BOHN, at Leipsick, made experiments which proved, that when the external light is wholly excluded, none can be seen in the cat's eye.

These experiments were favourable to the first opinion; but the brightness of the illumination is so great, that it appeared to exceed any effect which could be produced through the medium of the retina; so that some other source of light was thought necessary to account for the phænomenon: this circumstance gave support to the second opinion.

To determine which of the two opinions was just, several experiments were instituted, under the direction of Mr. RAMSDEN, who likewise assisted in making them. The truth of Professor BOHN's experiments was readily ascertained; it therefore only became necessary to inquire, whether the external light was of itself capable of producing so great a degree of illumination as that seen in the cat's eye.

This was attended with difficulty; for, when the apartment in which the experiments were made was so much darkened that nothing but the illumination from the eye was visible, the animal, by change of posture, or some other means, almost immediately deprived the observers of all light from that source. This was found to be the case, whether the cat, the tiger, or the hyena, was the subject of the experiment. On the other hand, when the light in the room was sufficient for the animal itself to be seen, the illumination in the eye was more obscure, and appeared to arise from the external surface of the iris.

As the difficulties which occurred in making observations on the illuminated state of the eye in the living animal were so great, an attempt was made to repeat, as nearly as possible, the experiments after death.

In doing so it was found, that a strong light thrown upon the cornea illuminated the iris, as it had done in the living eye; but, when the cornea was removed, this illumination disappeared. The iris was then dissected off, and the lucid tapetum completely exposed to view; the reflection from which was extremely bright; the retina proving no obstruction to the rays of light, but appearing equally transparent with the vitreous humour and crystalline lens.

From these experiments it appeared evident, that no light is generated in the eye; the illumination being wholly produced by the concave bright-coloured surface of the tapetum, collecting the rays of the external light, concentrated by the cornea and crystalline lens, and reflecting them through the pupil. When the iris is completely open, the degree of brilliancy is the greatest; but, when the iris is partly contracted, which it always is when the external light is increased, then the illumination is more obscure, and appears to come from the iris; a part of the light reflected from the tapetum being thrown back, by the concave surface of the cornea, upon the anterior surface of the iris, giving it a bright shining appearance.

The influence which the will of the animal has over this luminous appearance, seems altogether to depend on the contraction and relaxation of the iris. When the animal is alarmed or first disturbed, it naturally dilates the pupil, and the eye glares; when it is appeased or composed, the pupil contracts, and the light in the eye is no longer seen.

The most material information that has been gained in this investigation, is the transparent state of the retina in the eye during life; the opaque membranous appearance which it puts on in the dead body not being natural to it, but a change which takes place in consequence of death. This fact is almost all that is necessary, to explain the luminous appearance in the eyes of cats.

That neither Baron HALLER nor FONTANA had an adequate idea of the transparency of the retina, will appear from the following expressions respecting it, taken from their works:

HALLER describes it in the following words,

"Membranam crassam quidem, sed mollisimam, pellucidam "utique, quando recens oculus inspicitur, ut per eam sub aquis "choroideam videas; tamen ex flavo subcineream."* So that, although he calls it transparent, he says it is of a yellowish ash-colour.

FONTANA's expressions are, "Cette insensibilité de la rétine "à la lumière, en tant que lumière, dérive-t-elle de ce que les "nerfs sont encore trop gros, et ne sont pas bien découverts des

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* *Elementa Physiologiæ,* Tom. V. p. 385.

" tissus cellulaires ? ou de ce que la pulpe de la rétine est trop " amoncelée, et empêche les rayons de lumière d'arriver jusqu'à " ces mêmes nerfs ?" *

In considering the use of the lucid tapetum, it was an idea of the late Mr. HUNTER's, that the retina received a double stroke from the rays of light which entered the eye; one in passing to the tapetum, the other in returning from it.

This very ingenious opinion had some difficulties opposed to it, while the retina was supposed capable of obstructing the rays of light even in the smallest degree, as they could not be equably transmitted, so as to affect every part of the membrane alike. But the retina being ascertained to be absolutely transparent, these objections are entirely removed, and there can be no doubt that the rays of light, in those eyes which have a lucid tapetum, must remain upon the retina as long again as in the eyes of other animals; since the time required to strike upon the tapetum, and return, must be twice as much as is necessary for passing through the retina, to reach the nigrum pigmentum, where they are lost.

This may appear to be a consideration of little consequence, as the velocity of light is so great, and the continuance of impression necessary for distinct vision is that produced by a successive flow of similar rays of light from the object; it may, however, be all that is necessary for the purpose.

The retina being found perfectly transparent, when the eye is examined in a recent state, led to the idea that the internal structure of the optic nerve, when examined in the same state, might also be transparent. To ascertain this point, the following experiment was made:

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* *Sur le Venin de la Vipere,* 1781. Vol. II. p. 219.

The posterior half of a cat's eye, while in a very recent state, was immersed in a bason of water, and examined. The tapetum appeared very bright, the retina not having acquired sufficient opacity to become visible: the entrance of the optic nerve was a very white spot, which seemed to be opaque; but, when small pieces of coloured paper were alternately placed between the outside of the eye and the bottom of the bason, their colour was distinctly seen in the cavity of the eye, through the substance of the optic nerve; so that, at this part, the internal structure of the nerve has a degree of transparency.

This appeared to be a newly-discovered fact; and, to ascertain whether it was really so, the works of several physiological writers were consulted, but nothing was found which gave an idea that their authors had the smallest knowledge of it.

This semi-transparent state of the internal parts of the optic nerve, while recent, led naturally to the examination of its substance, by means of magnifying glasses; and, notwithstanding the failure of so many men of superior abilities, in this intricate inquiry, it held out the hope of meeting with some success.

The principal theories which have been formed respecting the structure of nerves, have been taken notice of by FONTANA: as they all differ from the observations which will be stated in the present Paper, it may not be improper to mention the heads of each of them, so as to bring into one point of view, all the knowledge that has been acquired on the subject.

TORRE found the medullary substance of the brain, spinal marrow, and nerves, to be a mass of transparent globules, swimming in a transparent fluid. When the parts were magnified one thousand times, the globules appeared largest in the brain, and smaller in the spinal marrow; they had no regular order: but, in the nerves, the globules were placed in lines, so as to give the appearance of fibres. In examining the optic nerve, the parts were magnified 120 times.

Prochaska considered the nerves to be composed of globules, united by a transparent elastic cellular membrane, and disposed in straight lines, resembling fibres.

Fontana found the primitive structure of nerves to consist of transparent cylinders, which, when united, formed the nerve: the manner of their being disposed is not mentioned. The objects were magnified 700 times, to show this appearance.

Dr. Monro considered the nerves as made up of spiral fibres; but afterwards found that what he had described was entirely an optical deception. In his last work, he says, "The optic "nerves have, in their whole course, less appearance of a fibrous "structure than perhaps any other pair of nerves in the human "body."

Other authors may have written upon this subject, and may have made observations upon the structure of nerves, but want of leisure must be an excuse for my not having come to a knowledge of them.

It is scarcely necessary to mention, that parts of an animal body are not fitted for being examined by glasses of a great magnifying power; and, wherever they are shewn one hundred times larger than their natural size, no dependance can be placed upon their appearance.

In making the following microscopical experiments on the internal structure of the optic nerve, great care was taken to avoid the errors of former inquirers. The microscope used was a single one; the focal length of the lens was about $\frac{35}{100}$

of an inch, so that the object was magnified about 23 times; and, that the results of the experiments might be as free from optical deceptions as the present state of our knowledge in this branch of science will admit, no appearance is described which Mr. RAMSDEN was not satisfied of having distinctly seen.

The experiments performed with the single microscope were repeated with a double one, made by Mr. RAMSDEN, which magnified the object about 40 times; but, in the double microscope, the appearances were indistinct, the reflection from the different glasses having thrown a confused glare upon the moist surface of the nerve. This circumstance led Mr. RAMSDEN to object to the use of compound microscopes, and to consider them as unfit for viewing objects of this kind.

For the following reasons, the optic nerve of the horse was selected, as the most proper for the experiments. It is of a large size, and several inches in length. It is readily procured in a recent state; as there are places in London where horses are allowed to be killed, and regular days in the week are fixed for that purpose.

That the examination of the nerve might be made as soon as possible after the animal's death, permission was procured from the man who superintends the killing of horses, to allow Mr. CLIFT to make the necessary experiments on the spot, the moment the horses were killed. Mr. CLIFT is the person intrusted with the care of keeping in order the late Mr. HUNTER's collection in comparative anatomy, and is well qualified, from his anatomical knowledge, and a familiarity in looking at organized parts through magnifying glasses, for an examination of this kind. These experiments were afterwards repeated by Mr. RAMSDEN and myself. From this mode of conducting them, the chances of error were few; since the person who first observed the appearances had no previous opinions on the subject; and Mr. Ramsden was better able than any other person, to correct such optical errors as might deceive Mr. Clift or myself.

The first experiments were made upon transverse sections of the nerve. One, near its termination in the eye, was placed upon glass, and exhibited in the microscope the following appearances: it was evidently composed of two parts, one opaque, the other transparent. The opaque portions were nearly circular in their shape, about 600 in number, and touched one another; the interstices between them were transparent.

When the opaque parts were attentively examined in a favourable light, and the nerve was in a recent state, they were found to be made up of a great number of smaller portions, each of which appeared to be also opaque. To see this subdivision of parts required some attention, and in many sections it could not be perceived. The cause of the difficulty seemed to be, the softness and tenacity of the substance divided, which therefore spread itself over the surface, giving it an uniform appearance; but, towards the circumference of the nerve, where the parts were cut obliquely, and some of them torn, the subdivision was very distinct. It was first observed by Mr. Clift, in several different sections; and was afterwards seen very distinctly, both by Mr. Ramsden and myself, in a nerve examined about two hours after death.

Having repeated these experiments six or seven times, on different days, so as to ascertain the accuracy of the results, the next object was, to determine whether the nerve had the same structure in its whole course. For this purpose, transverse sections were examined in different parts of the nerve, near the brain, towards the middle, and nearer the eye: of these experiments the following are the results. In all the sections, the nerve appeared to be made up of the same substances; but the size and number of the opaque parts differed very much. They have been stated, near the eye, to be 600; about the middle of the nerve, they were 150; and, near the brain, between the origin and union of the two nerves, they were only about 40. As they became larger, they were less regular in their shape, and had less of a circular form; nor were they uniform, some appearing very large, with one or two smaller placed between them.

After having succeeded in this examination of the nerve transversely, an attempt was made to investigate its structure in a longitudinal direction. To do this, a portion of the nervous pulp had its coat, formed by the dura mater, along with a thin vascular membrane which lines it, carefully removed for about an inch in length: the external surface of the pulp was then examined with a magnifying glass; the structure was evidently fasciculated, but the fasciculi did not run parallel to one another; they seemed to unite together and separate again, in such a manner that any one of them could not be traced for half an inch in length, without being lost in the neighbouring part. When thin sections were examined in the field of the microscope, they put on the same appearance: this was equally the case, whether the part examined was near the centre or circumference of the nerve. The fasciculi were largest in that part of the nerve near the brain, and smallest towards the eye. Great pains were taken to ascertain whether the fasciculi were made up of continued fibres, or of small parts unconnected, which, from their position, gave that appearance; but every observation that was made was in proof of their being continued fibres.

From these experiments, the internal structure of the optic nerve appears to be made up in the following manner:

At its origin from the brain, it consists of thirty or forty fasciculi or bundles of extremely small opaque pulpy fibres, the interstices between which are filled with a transparent jelly. As the nerve goes farther from the brain, the fasciculi form smaller ones, of different sizes. This is not done by a regular subdivision, but by a few fibres going off laterally from several large fasciculi, and being united, forming a smaller one: some of the fasciculi so formed, which are very small, unite again into one. In this way, the fasciculi gradually diminish in size, and increase in number, till they terminate in the retina.

Near the eye, where the fasciculi are most numerous, the substance of the nerve has a considerable degree of transparency, from the number of transparent interstices between them; but this is less the case nearer the brain, where the interstices are fewer.

In the optic nerve of the cat, the structure is the same as in the horse; but, from the smallness of the parts, less fitted for investigation. Near the eye, its internal substance is more transparent than the corresponding part in the horse.

To see how far this structure was peculiar to the optic nerve, similar experiments were made upon the internal substance of the fifth and seventh pair of nerves, near their origin at the brain, and the structure was found to be the same. In these last mentioned nerves, the interstices between the fasciculi were smaller than in the optic nerve, rendering their transverse sections less transparent; from which it is natural to suppose, that the internal parts of the optic nerve are not so compact as in other nerves, and therefore it is better fitted for examination.

These experiments show, that the nerves do not consist of tubes conveying a fluid, but of fibres of a peculiar kind, different from every thing else in the body, with which we are acquainted. The course of these fibres is very curious; they appear to be constantly passing from one fasciculus to another, so as to connect all the different fasciculi together by a mixture of fibres. This is different from the course of blood-vessels, lymphatics, or muscular fibres: the only thing similar to it, is in the formation of nervous plexuses; which leads to the idea of its answering an essential purpose, respecting the functions of the nerves.

# II. *The* Bakerian *Lecture. Observations upon an unusual horizontal Refraction of the Air; with Remarks on the Variations to which the lower Parts of the Atmosphere are sometimes subject. By the Rev.* S. Vince, *A. M. F. R. S. and Plumian Professor of Astronomy and experimental Philosophy, in the University of Cambridge.*

Read November 15, 1798.

THE uncertainty of the refraction of the air near the horizon has long been known to astronomers, the mean refraction varying by quantities which cannot be accounted for from the variations of the barometer and thermometer; on which account, altitudes of the heavenly bodies which are not more than 5° or 6°, ought never to be made use of when any consequences are to be deduced from them. The cause of this uncertainty is probably the great quantities of gross vapours, and exhalations of various kinds, which are suspended in the air near to the earth's surface, and the variations to which they are subject; causes, of which we have no instruments to measure the effects which they produce, in refracting the rays of light. In general, the course of a ray passing through the atmosphere, is that of a curve which is concave towards the earth, the effect of which is to give an apparent elevation to the object; and thus the heavenly bodies appear above the horizon, when they are actually below it; but it will not alter the position of their parts, in respect to the horizon, that is, the image of the highest part of the object will be uppermost, and the image of the lowest part will be undermost. The figures, however, of the sun and moon, when near the horizon, will suffer a change, in consequence of the refraction of the under limb being greater than that of the upper; from which they assume an elliptical form, the minor axis of which is perpendicular to the horizon, and the major axis parallel to it. But a perpendicular object, situated upon the surface of the earth, will not have its length altered by refraction, the refraction of the bottom being the same as that of the top.* These are the effects which are produced upon bodies at or near the horizon, in the common state of the atmosphere, by what I shall call the *usual* refraction.

But, besides the usual refraction which affects the rays of light, the atmosphere over the sea is sometimes found to be in a state which refracts the rays in such a manner as to produce other images of the object, which we will call an effect from an *unusual* refraction. In the Phil. Trans. for 1797, Mr. HUDDART has described some effects of this kind, which he has accounted for by supposing that, from the evaporation of the water, the refractive power of the air is not greatest at the surface of the sea, but at some distance above it; and this will solve, in a very satisfactory manner, all the phænomena which he has observed. But effects very different from those which have been described by Mr. HUDDART are sometimes found to take place. These I had an opportunity of observing at Ramsgate, last summer, on August the first, from about half an hour after four o'clock in the afternoon till between seven and eight. The day had been extremely hot, and the evening was very

* See my *Complete System of Astronomy*, Art. 194.

sultry; the sky was clear, with a few flying clouds. I shall describe the phænomena as I observed them with a terrestrial telescope, which magnified between 30 and 40 times; they were visible, however, to the naked eye. The height of the eye, above the surface of the water, at which most of the observations were made, was about 25 feet; some of them, however, were made at about 80 feet from the surface; and it did not appear that any of the phænomena were altered from varying the height of the eye, the general effect remaining the same.

The first unusual appearance which I observed, was that which is represented in Tab. I. fig. 1. Directing my telescope at random, to examine any objects which might happen to be in view, I saw the top of the masts of a ship A, above the horizon, $x y$, of the sea, as shown in the figure; at the same time also, I discovered in the field of view, two complete images, B, C, of the ship in the air, vertical to the ship itself, B being inverted, and C erect, having their hulks joined. The phænomenon was so strange, that I requested a person present to look into the telescope, and examine what was to be seen in it, who immediately described the two images, as observed by myself; indeed they were so perfect, that it was impossible we could differ in our description. Upon this, I immediately took a drawing of the relative magnitudes and distances of the ship and its images, which, at that time, were as represented in the figure, as near as it was possible for the eye to judge; and it was very easy to estimate them to a very considerable degree of accuracy. As the ship was receding from the shore, less and less of its masts became visible; and, continuing my observations, in order to discover whether any, or what variations might take place, I found that, as the ship descended, the images B, C ascended; but, as the ship did not sink below the horizon, I had not an opportunity of observing at what time, and in what order, the images would have vanished, if the ship had so disappeared.

Being desirous of seeing whether the same effect was produced upon the other ships which were visible, I directed my telescope to another ship A, (fig. 2.) whose hulk was just in the horizon $x\,y$; when I observed a complete inverted image B, the main-mast of which just touched that of the ship itself. In this case, there was no second image as before. The ship A moving upon the horizon, B continued to move with it, without any variation in its appearance.

The next ship which I directed my telescope to, was so far on the other side of the horizon $xy$, as just to prevent its hulk from being seen, as is represented by A, (fig. 3.). And here I observed only an inverted image of part of the ship; the image $y$ of the topsail, with the mast joining that of the ship, the image $x$ of the top $a$ of the other mast, and the image $z$ of the end $c$ of the bowsprit, only appearing at that time. These images would suddenly appear and disappear very quickly after each other; first appearing below, and running up very rapidly, showing more and less of the masts at different times, as they broke out; resembling, in the swiftness of their breaking out, the shooting out of a beam of the Aurora borealis. As the ship was descending on the other side of the horizon, I continued my observations upon it, in order to discover what changes might take place; when I found, that as it continued to descend, more of the image gradually appeared, till at last the image of the whole ship was completed, with their main-masts touching each other; and, upon the ship descending lower, the image and the ship separated; but I observed no second image, as in the first case; a second image, however, might probably have appeared, if the ship had continued to descend.

Upon moving my telescope along the horizon, in order to examine any other ships which might be in sight, I observed, just at the horizon *x y*, (in fig. 4.) the top *a* of the mast of a ship; and here an effect was observed, which had not been before discovered; for there was an inverted image B, vertical to *a*, an erect image C, both of them very perfect and well defined, and an image *v w* of the sea between them, the water appearing very distinctly. As the ship was coming up towards the horizon, I continued to observe it, in order to discover the variations which might follow, and found, that as the ship approached the horizon, the image C gradually disappeared, and at last it vanished; after that, the image *v w* of the sea disappeared; and during this time the image B descended; but the ship did not rise so near to the horizon as to bring the main-masts together. Had I directed my telescope to the same point of the horizon a little sooner, I should have seen the two images, before the ship itself was visible. In fact, the images were visible, when the whole ship was actually below the horizon; for, from the very small part of the mast which was at first visible, that part must then have been below the horizon, and appeared above it by the usual refraction; the altitude of *a*, above the horizon, having then been much less than the increase of altitude which arises from the common horizontal refraction. The discovery of ships in this manner might, in some cases, be of great importance; and, on such occasions, it might be worth while to appoint proper persons to make observations for that purpose.

The cliffs at Calais being very visible, I directed my telescope towards them, in order to examine whether there was any thing unusual in their appearance; when I observed an image of the cliffs, above the cliffs themselves, together with an image of the sea separating them, as is represented in fig. 5.; in which, $x\,y$ represents the horizon of the sea, AB the cliffs, $a\,b$ their image, and $v\,w$ the image of the sea between them: the depth of $a\,b$ was much less than that of AB. It is probable, however, that $vw$ might not be the image of the sea immediately adjoining to the cliffs, but a partial elevation of the sea at some distance from them; and that the image $v\,w$ might intercept some part of the image $a\,b$, which would otherwise have been visible: we must not therefore conclude, that the image $a\,b$, so far as it appeared, was less than the corresponding part of the object. From the memorandums which I made at the time of observation, I do not find that I examined the appearance of the cliff AB, and its image $a\,b$; which, had there at that time been any striking marks in them, would have determined whether the object and its image were of the same magnitude. The image $a\,b$ was, however, erect; the boundaries on the top of AB and $a\,b$ agreeing together. Having examined this for some time, and taken a drawing of the appearance, during which I could discover no variation, I directed my telescope to other objects; and, upon turning it again to the same cliffs, after the space of about six or seven minutes, the images $a\,b$ and $vw$ were vanished; but, examining them again soon after, the images were again visible, and in every respect the same as they appeared before. A short time after, they disappeared, and did not appear any more.

Soon after the above appearances, I observed a ship C, with the hulk below the horizon *x y*, passing by the same cliffs AB; an inverted image D of which appeared against the cliffs, as represented in fig. 6. The ship was in motion, and remained at the same distance on the other side of the horizon: I continued my observations upon it till it had passed the cliffs for a considerable distance, but there was no change of appearance. The cliffs were illuminated by the sun, and appeared very distinctly; but there was no image above, as in the last case.

Continuing to observe the same cliffs AB, fig. 7, I soon after discovered two partial elevations *m*, *n*, of the sea, by the unusual refraction; they changed their figures a little, and disappeared in the place where they first appeared, and were equally distinct in every part.

About this time, I observed a very thick fog coming upon the horizon from the other side, rolling upon it with a prodigious velocity; curling as it went along, like volumes of smoke sometimes out of a chimney. This appeared several times. I conclude, therefore, that there was a considerable fog on the other side of the horizon.

The last phænomenon which I observed was that which is represented in fig. 8; where *x y* represents the horizon, *a b* two partial elevations of the sea, meeting at *c*, and continued to *d*; *e*, another partial elevation of the sea, of which kind I observed several, some of which moved parallel to the horizon, with a very great velocity. I conjecture, therefore, that these appearances were, in part at least, caused by the fog on the other side of the horizon. For, though I did not at the same time see the motion of these images and that of the fog, yet, from memory, I judged the motions to be equal; and they were also in the same direction. A fog which, by producing an unusual refraction, might form these images, would, by its motion, produce a corresponding motion of the images.

I have here described all the different phænomena which I observed from the unusual refraction, of most of which I saw a great many instances. Every ship which I observed on the other side of the horizon of the sea, exhibited phænomena of the kind here described, but not in the same degree. Of two ships which, in different parts, were equally sunk below the horizon, the inverted image of one would but just begin to appear, whilst that of the other would represent nearly the whole of the ship. But this I observed, in general, that as the ship gradually descended below the horizon, more of the image gradually appeared, and it ascended; and the contrary, when the ships were ascending. Upon the horizon, in different parts, one ship would have a complete inverted image; another would have only a partial image; and a third would have no image at all. The images were in general extremely well defined; and frequently appeared as clear and sharp as the ships themselves, and of the same magnitude. Of the ships on this side of the horizon, no phænomena of this kind appeared. There was no fog upon our coast; and the ships in the Downs, and the South Foreland, exhibited no uncommon appearances. The usual refraction at the same time was uncommonly great; for the tide was high, and at the very edge of the water I could see the cliffs at Calais a very considerable height above the horizon; whereas they are frequently not to be seen in clear weather from the high lands about the place. The French coast also appeared both ways, to a much greater distance than I ever observed it at any other time; particularly towards the east, on which part also the unusual refraction was the strongest.

During the remainder of my stay at Ramsgate, which was about five weeks, I continued daily to examine all the ships in sight; but I discovered no phænomena similar to those which I have here given a description of. The phænomenon of the ship observed by Mr. HUDDART, differed altogether from those above described, as the inverted image which he observed was below the ship itself. An appearance of this kind I observed on August the 17th, about half an hour after three o'clock in the afternoon, of which fig. 9. is a representation. The real ship is represented by A, and the image by B; *er*, *mv*, the hulks; *s t* the flag, and *w x* its image, just touching it, with the sea *xy* below. Between the two hulks, some faint dark spots and lines appeared, but I could not discover what they were the representatives of. The vessel, at the time of this appearance, was not quite come up to the horizon; and, as it approached it, the image gradually diminished, and totally disappeared when the ship arrived at the horizon.

It remains now, that we inquire into the causes which might produce the very extraordinary effects which have been above related. From the phænomena, we are immediately led to the nature of the path of the rays of light to produce them; and we may conceive, that the air may possibly be in such a state as will account for the unusual tract which they must have described. For, let $b z$ (fig. 10.) be the surface of the sea; $a b$ an object; E the place of the eye; $a r$ E, $b s$ E, the progress of two rays, by the usual refraction, from the extreme parts of the object to the eye; to these curves draw the tangents E$a'$, E$b'$, and $a' b'$ will be the image of the object, as usually formed. Now, if we take the case represented in fig. 4, let $a'' b''$ represent the inverted image, and $a''' b'''$ the erect image; join $a''$ E,

$a'''$ E, and $b''$ E, $b'''$ E, and these lines must respectively be the directions of the rays entering the eye from $a$ and $b$, in order to produce the images $a'' b''$ and $a''' b'''$; hence, these lines must be tangents at E, to the curves which are described by the rays of light; let therefore $a\,n$ E, $a\,m$ E, $b\,v$ E, $b\,w$ E, be the curves described. We have therefore to assign a cause which may bring rays passing above the rays $a\,r$ E, $b\,s$ E, to the eye at E. Now, if there were no variation of the refractive power of the air, a ray of light passing through it would describe a straight line; therefore, the curvature of a ray of light passing through the atmosphere, depends upon the *variation* of the refractive power of the air. If, therefore, we suppose the air lying above $a\,r$ E, to vary quicker in its refractive power than the air through which $ar$E passes, the curvature of a ray proceeding above that of $a\,r$E, will be greater than the curvature of $a\,r$ E; and upon this principle we may conceive that a ray may describe the curve $a\,n$ E: and, in like manner, if a quicker variation of refractive power should take place above the curve $an$E, than in that curve, a third ray may describe the curve $a\,m$ E. The same may be said for the rays $bv$E, $bw$E, diverging from $b$. The alterations of the refractive power may arise, partly from the variations of its density, and partly from the variations of its moisture; and the passage of the rays through the boundary of the fog may there suffer a very considerable refraction; for, from the motion of the fog, and that of the images abovementioned, I have no doubt that the fog was a very considerable agent in producing the phænomena. When all the causes co-operate, I can easily conceive that they may produce the effects which I have described. If the cause should not operate in the tract of air through which the curves

Fig. 1.

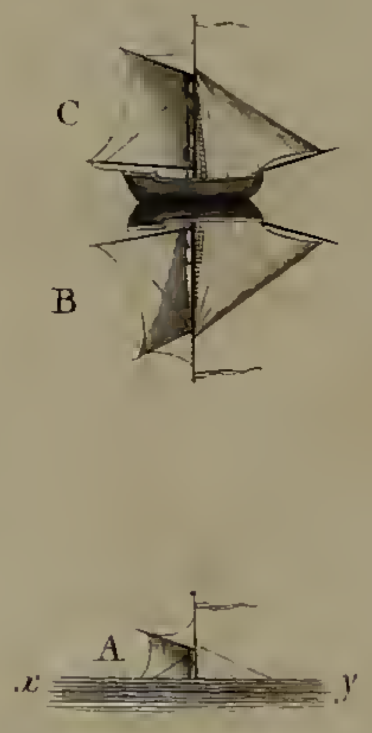


Fig. 2.

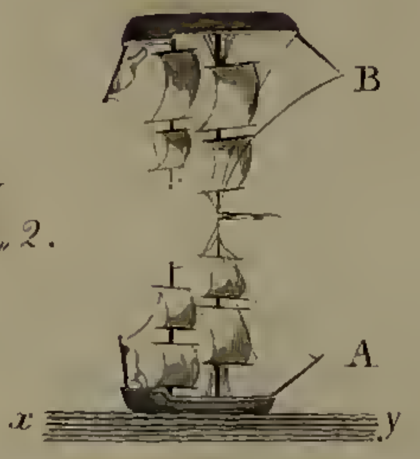


Fig. 3.

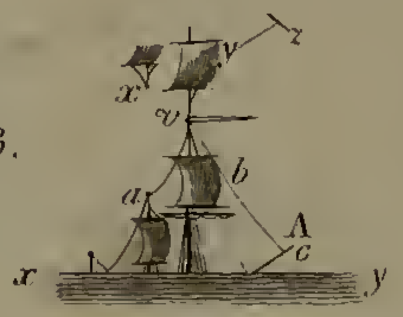


Fig. 4.

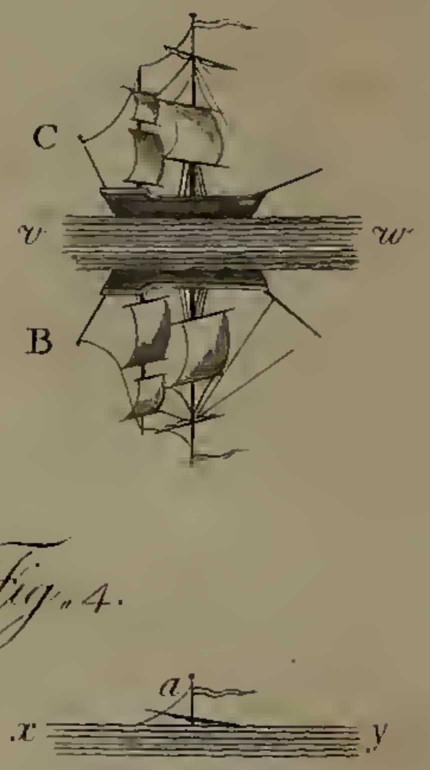


Fig. 5.

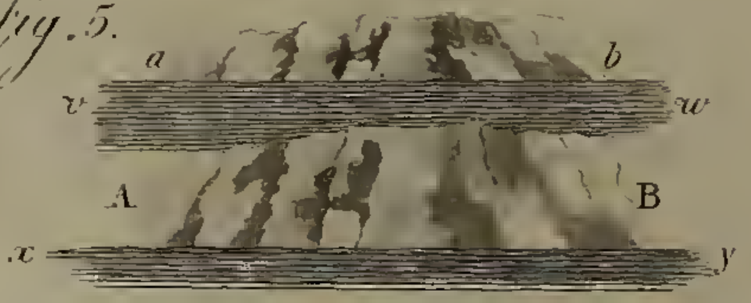


Fig. 6.

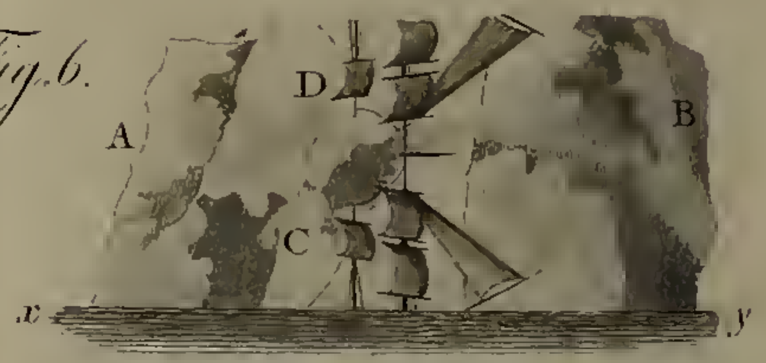


Fig. 7.

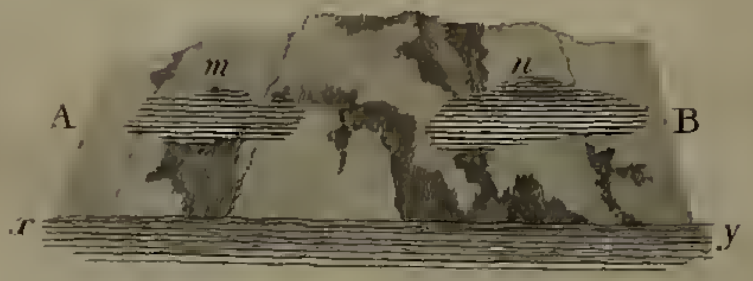


Fig. 8.

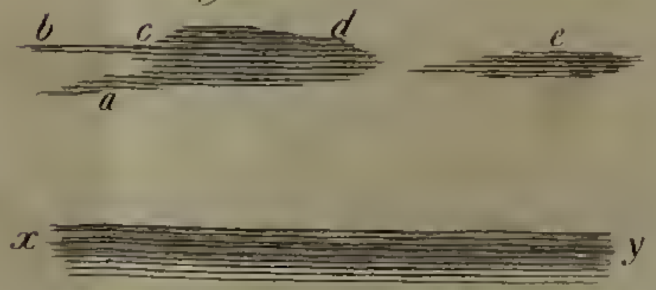


Fig. 9.

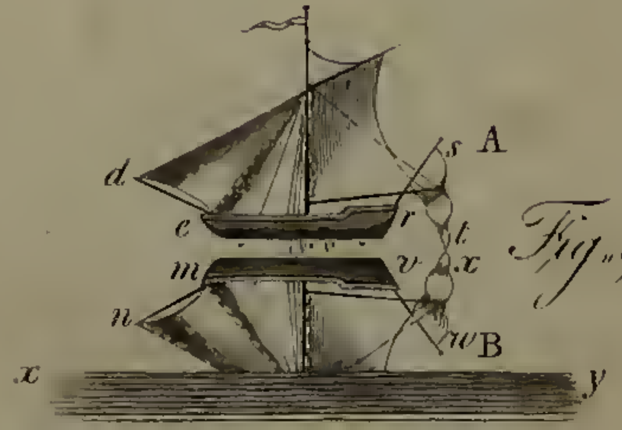
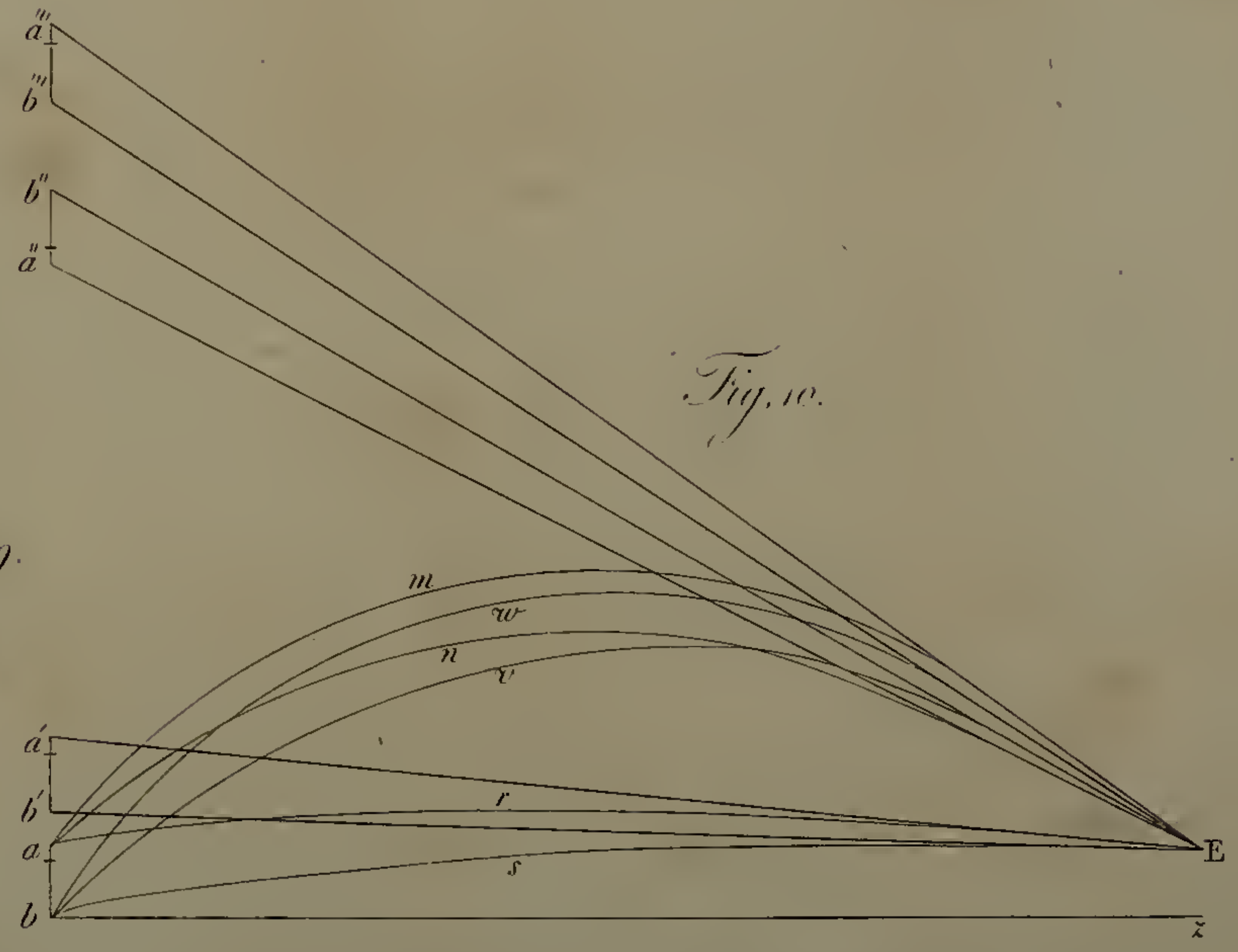


Fig. 10.



$anE$, $bvE$ pass, but should operate in the tract through which $amE$, $bwE$ pass, an erect image would be visible, but there would be no inverted image; and, should it operate in the latter case, but not in the former, there would be only an inverted image.

As the phænomena are very curious, and extraordinary in their nature, and have not, that I know of, been before observed, I have thought proper to lay a description of them, with all the attending circumstances, before the Royal Society. They appear to be of considerable importance; as they lead us to a knowledge of those changes to which the lower parts of the atmosphere are sometimes subject. If, when these phænomena appear, a vessel, furnished with a barometer, thermometer, and hygrometer, below, and also at the top of the mast, were sent out to pass below the horizon and return again, and an observer at land, having like instruments, were to note, at certain intervals, the situation and figure of the images, it might throw further light upon this subject, and lead to useful discoveries respecting the state of the atmosphere, from a conjunction of the causes which affect these instruments.

# III. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1797. *With some Remarks on the Recovery of injured Trees. By* Thomas Barker, *Esq. Communicated by Mr.* Timothy Lane, *F. R. S.*

Read December 13, 1798.

| | | Barometer. | | | Thermometer. | | | | | | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | In the House. | | | Abroad. | | | |
| | | Highest. | Lowest. | Mean. | High. | Low. | Mean. | High. | Low. | Mean. | |
| | | Inches. | Inches. | Inches. | ° | ° | ° | ° | ° | ° | Inches. |
| Jan. | Morn. | 29,99 | 29,01 | 29,58 | 46 | 33 | 39 | 48 | 26 | 37 | 1,319 |
| | Aftern. | | | | 48 | 36 | 40 | 51 | 29 | 39 | |
| Feb. | Morn. | 30,07 | 28,84 | 50 | 47 | 37 | 42 | 46½ | 25 | 34 | 0,076 |
| | Aftern | | | | 48 | 38½ | 43½ | 52½ | 32 | 42½ | |
| Mar. | Morn. | 29,90 | 29,03 | 47 | 46 | 38 | 41½ | 46 | 29½ | 35½ | 0,908 |
| | Aftern. | | | | 47 | 39 | 43 | 53 | 36½ | 44½ | |
| Apr. | Morn. | 29,70 | 28,63 | 31 | 51½ | 42½ | 46 | 52 | 36 | 42½ | 2.882 |
| | Aftern. | | | | 52½ | 43½ | 47 | 59 | 37 | 49 | |
| May | Morn. | 29,86 | 28,44 | 38 | 67 | 44 | 53 | 64 | 38 | 50½ | 2,528 |
| | Aftern. | | | | 69 | 44½ | 54½ | 76 | 41½ | 59½ | |
| June | Morn. | 29,85 | 28,93 | 41 | 60 | 52 | 57 | 63 | 39½ | 52½ | 4,221 |
| | Aftern. | | | | 62½ | 53½ | 58 | 76 | 43½ | 63 | |
| July | Morn. | 29,80 | 29,05 | 49 | 70½ | 57 | 63 | 66 | 54 | 61 | 3,075 |
| | Aftern | | | | 72½ | 59 | 65½ | 83 | 60 | 72½ | |
| Aug. | Morn. | 29,72 | 29,05 | 35 | 64 | 58 | 60½ | 64 | 49 | 58 | 2,415 |
| | Aftern. | | | | 65½ | 59 | 62 | 76 | 62 | 68 | |
| Sep. | Morn. | 29,67 | 28,67 | 28 | 62 | 51½ | 56 | 58 | 43 | 50 | 4,792 |
| | Aftern. | | | | 64 | 54 | 57 | 70½ | 51 | 60 | |
| Oct. | Morn. | 29,83 | 28,57 | 37 | 55½ | 41½ | 49½ | 55 | 31½ | 44 | 1,143 |
| | Aftern. | | | | 57½ | 42½ | 50 | 63 | 42½ | 52 | |
| Nov. | Morn. | 29,98 | 28,50 | 43 | 49 | 34 | 43½ | 51½ | 24½ | 38½ | 1,620 |
| | Aftern. | | | | 49½ | 34½ | 44 | 53 | 30 | 44 | |
| Dec. | Morn. | 29,93 | 28,60 | 27 | 49½ | 36 | 42 | 53 | 29 | 38½ | 2,875 |
| | Aftern. | | | | 49½ | 36½ | 42½ | 53 | 32 | 42 | |
| | | | | | | | | | | | 27,854 |

After a few misty days, the beginning of the year was frosty, winterly, and severe, yet not so hard as before; with frequent but little rains. The end of January, all February, and beginning of March, there was hardly any rain at all; a calm, dry, and pleasant time, but not forwarding, for there was frequent frost, scarcely going off in the shade all day, yet the noontide sun was very warm. The greater part of March was dry, with frequent frosty mornings, and very backward, though calm; but the last ten days were showery, pleasant, and forwarding: this was the beginning of a very wet half year. It was a blasting spring; and some fruits were cut off, after they seemed set, and considerably grown; yet some orchards escaped, and the frequent wet made strawberries plentiful. The season continued cold, wet, and backward, in general, but the last week in April was warmer and more forwarding, yet still wet; then it turned cold again, but there were a few hot dry days toward the end of May. It was wet again at the end of that month; very much so, almost all June, and the beginning of July, with several unseasonable floods, and cold at times; but the rest of July was fairer for some time, and was the best and hottest part of the summer, till a very great thunder storm on the 30th. There was a great crop of hay, but bad weather for getting it up, and in the low meadows it was much flooded. The harvest was good, as to quantity, but could not be all well got, on account of the bad season, yet there was much less complaint of grown corn than might have been expected; perhaps, what was not well got, might be used, not so much for bread, as for starch, powder, and distilling; for there was a vast difference in the price of good and bad wheat, some sorts being almost twice as much as others. Most sorts of fruit were scarce; but peaches and nectarines were more plentiful, and finer, than either apricots before, or apples after them. I think the pears and apples were more defective in size than in number, for they were remarkably small. The harvest was all showery; but in August the weather was more moderate and tolerable than in September, when there were several great rains and storms.

The autumn, beginning with a few of the last days of September, was in general fine, fair, open, and mild, with moderate winds, and when calm, perfectly so; a good wheat seed time, which came up well; but it was wetter again the end of November, and in December, which kept the ground (already much soaked by the very wet summer) from drying so much as it otherwise might have done. It continued, however, an open winter, not much frost, and that not severe.

### *On the Recovery of injured Trees.*

About the year 1788 or 1789, a Lucombe oak was planted, the top of which might be about six feet high, but was broken off in coming down. In spring, the tree put out at the two highest buds, but much better at some lower ones. In 1791, the two highest buds again put out, yet, as before, very indifferently; however, a lower bud, about five feet high, put out a strong shoot, about fifteen inches long; but, as side branches are apt to do, it did not grow upright, but slanting. In winter, I fixed that strong shoot upright, by tying it to another shoot, which came out of the opposite side of the tree; and, in 1792, it made a very strong, straight, and upright shoot, three feet nine inches long, and as thick as a moderate finger, and has continued thriving ever since. The tree is now about eighteen feet high, and eleven inches in girth at the bottom; it has been pretty well cleared of boughs about half way up, and may perhaps, by degrees, be cleared several feet higher.

In the winter of 1789, an ash tree was cut down, which, falling against a young oak, as thick as my leg, beat it down. I had the oak cut close to the ground, and in 1790 it put out a number of shoots, which grew that year, and 1791. In 1792, I chose out the best shoot, trained it up as straight as I could, and beat down the rest of the shoots to the ground, under the hedge, to weaken them, and encourage the best shoot, which I intend to be the tree. It has since grown strong, is pretty straight, has been pruned, and may, I believe, by degrees, be cleared to a good height, for the leading bud is strong and upright. It is now (in August, 1798) about 14½ feet high, and about 6½ inches in girth at the bottom.

# IV. *Some Additions to a Paper, read in* 1790, *on the Subject of a Child with a double Head. By* Everard Home, *Esq. F. R. S.*

Read December 13, 1798.

In the year 1790, I laid before this learned Society, an account of a child with a double head, illustrated by drawings, which is honoured with a place in the Philosophical Transactions, Vol. LXXX.

Since that time, Mr. Dent, the gentleman who sent over from India the double skull, which was shewn at the meeting when the Paper was read, has returned to England. Among his drawings there are two portraits of the double head, taken by Mr. Devis, an artist of considerable merit, who was upon a visit at Mr. Dent's house, in Bengal, when the child was brought there alive, to be shewn as a curiosity. These drawings give a more faithful representation of the appearance of the double head, than the engravings annexed to the former Paper, and at the same time exhibit a striking likeness of the child's features.

Mr. Dent has given permission to have them copied; and, from a desire to render as complete as possible, the account and representation of so curious and rare an instance of deviation from the natural structure in the human body, they are now offered to the Society, (see Tab. II. and III.) with some observations made by Mr. Dent, as a supplement to the former Paper.

Mr. DENT's observations, in addition to those already in the possession of the Society, are the following.

The child was a male. Its father was a farmer at Mundul Gaut, in the province of Bardwan, who told Mr. DENT, that it was more than four years old at the time of its death.*

The mother, who was thirty years of age, had three children, all naturally formed; and her fourth child was the subject of the present Paper. Mr. DENT endeavoured to discover whether any imaginary cause had been assigned by the parents, for the unnatural formation of the child; but the mother declared, that no circumstance whatever, of an uncommon nature, had occurred: she had no fright, met with no accident, and went through the period of her pregnancy exactly in the same way as she had done with her other children.

The body of the child was uncommonly thin, appearing emaciated from want of due nourishment.

The neck of the superior head was about four inches long; and the upper part of it terminated in a hard, round, gristly tumour, nearly four inches in diameter.

The front teeth had cut the gums, in the upper and under jaws of both heads.

When the child cried, the features of the superior head were not always affected; and, when it smiled, the features of the superior head did not sympathize in that action.

In preparing the skull, which unpleasant operation Mr. DENT was obliged, from the prejudices of his servants, to superintend, he found that the dura mater belonging to each brain, was continued across, at the part where the two skulls joined, so that each brain was invested, in the usual way, by its own proper coverings; but the dura mater which covered the cerebrum of the upper brain, adhered firmly to the dura mater of the lower brain: the two brains were therefore separate and distinct, having a complete partition between them, formed by an union of the duræ matres.

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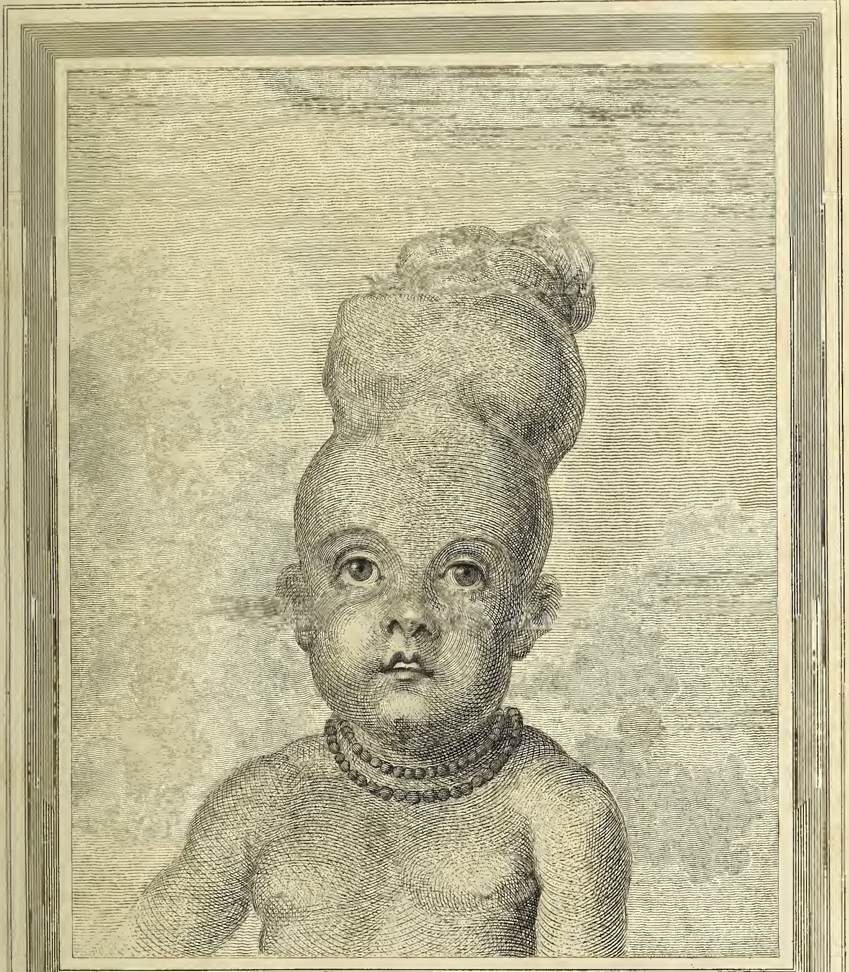
* In the former Account, the child is said to have been about two years old at that time.

When the contents of the double skull were taken out, and this union of the duræ matres more particularly examined, a number of large arteries and veins were seen passing through it, making a free communication between the blood vessels of the two brains. This is a fact of considerable importance, as it explains the mode in which the upper brain received its nourishment.

Before these observations were communicated by Mr. DENT, it was natural to suppose that the two brains had been united into one mass; as it was difficult to imagine in what way the upper brain could be supplied with blood.

*Philos. Trans.* MDCCXCIX. *Tab.* II. *p.* 30.



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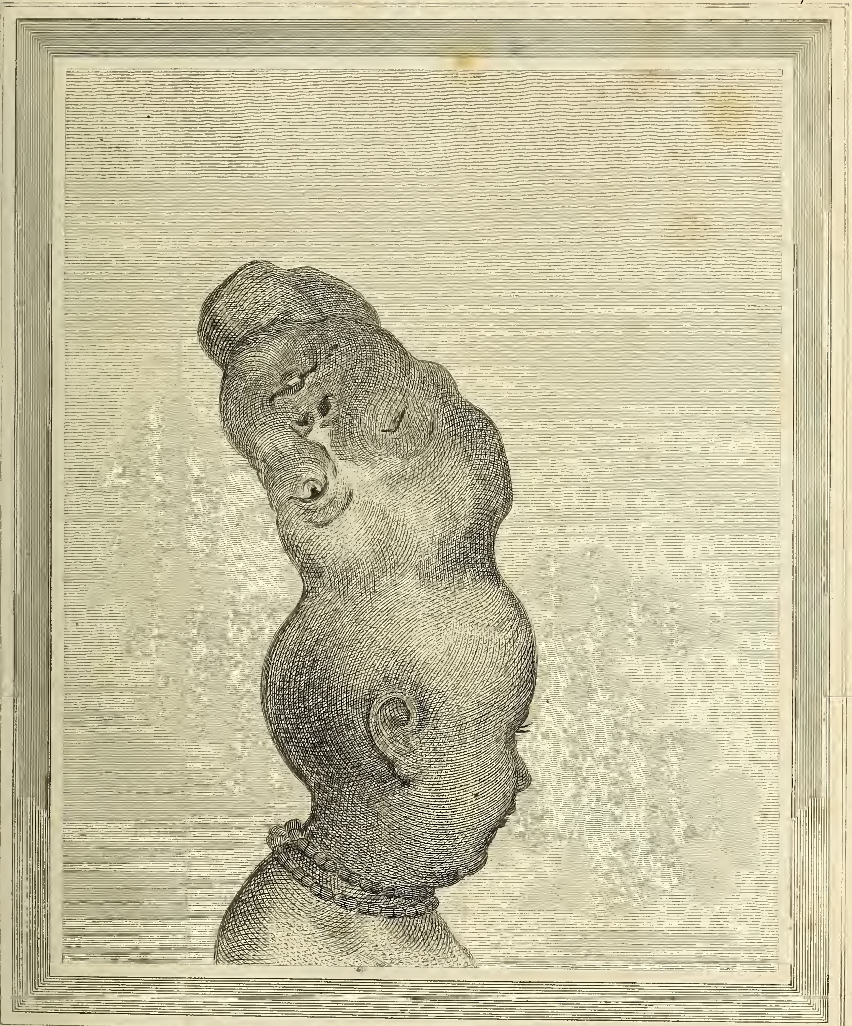
BRITISH MUSEUM NATURAL HISTORY



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BRITISH MUSEUM NATURAL HISTORY.

## V. *Observations on the Manners, Habits, and Natural History, of the Elephant. By* John Corse, *Esq. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read January 24, 1799.

Since the remotest ages, the elephant, on account of his size, his sagacity, and his wonderful docility, has attracted the notice, and excited the admiration, of philosophers and naturalists, both ancient and modern; and few travellers into Asia, or Africa, have omitted giving some account of him.

A residence, however, of more than ten years, in Tiperah, a province of Bengal, situated at the eastern extremity of the British dominions in Asia, where herds of elephants are taken every season, afforded me frequent opportunities of observing, not only the methods of taking them, but also the habits and manners of this noble animal.

From the year 1792 till 1797, the elephant hunters were entirely under my direction; so that I had it in my power to institute such experiments as I thought likely to discover any particulars, not formerly known, in the natural history of the elephant. Soon after my arrival at Tiperah, while informing myself of the methods of taking wild elephants, I had occasion to observe, that many errors, relative to the habits and manners of that useful animal, had been stated in the writings of European authors, and countenanced by some of the most approved writers.

The elephant has been declared to possess the sentiment of modesty in a high degree; and, by some, his sagacity was supposed to excite feelings for the loss of liberty, so acute, as to cause him to refuse to propagate his species while in slavery, lest he should entail on his progeny a fate similar to his own; whilst others have asserted, that he lost the power of procreation in the domestic state.

So circumstanced, I was desirous of taking advantage of my situation, and of making such experiments and observations, as might tend to render more perfect the natural history of this useful animal.

Early in the year 1789, I gave an account of the methods then used for taking and training wild elephants, to the Asiatic Society in Calcutta, which was published in Vol. III. of their Researches; and the following experiments and observations, made since that period, on the natural history of the elephant, will not, I hope, prove unworthy the attention of the Royal Society.

The young of the elephant, at its birth, is about 35 inches high; and, as a knowledge of its progressive growth forms the best criterion by which we can judge of the age of this animal, I shall here note down some observations made on this subject, till the elephant has attained its full size; for, after this period, till signs of old age appear, I do not know any marks by which a tolerable guess can be made of the number of its years, unless we could examine the teeth accurately; and, even then, there would be much uncertainty.

Very erroneous notions have been entertained, with respect to the size of elephants, in different parts of India; for which reason, I have collected such facts as were likely to ascertain their general height. The following observations, of the gradual increase of growth, were made upon a young elephant of Mr. Stephen Harris, which was accurately measured from time to time, and upon a female elephant of my own, till I left Tiperah.

Mr. Harris's elephant, at its birth, October 16, 1789, was 35 inches high.

| | | Feet. | Inches. |
|---|---|---|---|
| In one year he grew | 11 inches, and was | 3 | 10 high. |
| In the 2d year | 8 - - | 4 | 6 |
| In the 3d year | 6 - - - | 5 | 0 |
| In the 4th year | 5 - - | 5 | 5 |
| In the 5th year | 5 - - - | 5 | 10 |
| In the 6th year | 3½ - - | 6 | 1½ |
| In the 7th year | 2½ - - | 6 | 4 |

Except during his 4th and 5th years, the above measurement shows a gradual decrease in the proportion of growth for every year; and there was no opportunity of tracing the growth of this elephant further than its 7th year.

Another elephant, six feet nine inches high, at the time she came into my possession, was supposed to be fourteen years old; but, as the accuracy of the hunters cannot be depended on, it will be proper to take Mr. Harris's elephant, whose age is exactly known, as a standard; and, judging from its annual increase, this will lead us to consider the elephant, at the time I received her, to be only eleven years old; giving a period of four years, for the addition of five inches. I have made a greater allowance of time, on account of this elephant being a female, and Mr. Harris's a male, which there is much reason to believe grows faster.

During the next five years, before she was covered, she grew only six inches; but, what is extremely curious, while pregnant, she grew, in twenty-one months, five inches; and, in the following seventeen months, though again pregnant, she grew only half an inch; at which time, she was sent from Comillah, as I was then preparing to leave India.

At this time, she was about 19 years old, and had perhaps attained her full growth. Her young one was then (Nov. 1796) not twenty months old; yet he was four feet five inches and a half high, having grown eighteen inches since his birth; which is the greatest progressive growth, in the elephant, that I have known.

These observations, when applied to the general growth of elephants, are to be taken with some allowance; since, during the state of the first pregnancy, there is so great an irregularity in the growth of female elephants, as alone occasions considerable difficulty, even supposing the progressive growth nearly equal in the species. It is probable, however, that this is not by any means equal: for, as elephants vary greatly in size, and as males are generally much taller than females, we must conclude they either grow faster, or are longer in attaining their full growth.* But it may be safely asserted, that elephants, like most quadrupeds, propagate their species before they have acquired their full growth. Many females have been known, when taken while pregnant, to have grown several inches higher before delivery; and, as it has been stated, that the female elephant on which my observations were made, could not exceed 16 years when she received the male, it is probable the wild female elephants are in heat before that period.

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* A male elephant, belonging to the Cudwah Rajah till he was above twenty years of age, continued to increase in height, and was supposed not to have attained his full size, when I left Tiperah: he was then about twenty-two years old.

If, from the above data, it may be allowed to form a probable conjecture, elephants attain their full size between eighteen and twenty-four years of age. The height of the elephant, I believe, has been generally much exaggerated. In India, the height of females is, in general, from seven to eight feet; and that of males, from eight to ten feet, measured at the shoulder.

I have never heard but of one elephant, on good authority, that much exceeded ten feet: this was a male, belonging to Asoph ul Dowlah, the late vizier of Oude. His dimensions, as obligingly communicated to me by Mr. Cherry, then Resident at Lucknow, were as follow.

He was measured on the 18th of June, 1796.

| | Feet. | Inches. |
|---|---|---|
| From foot to foot, over the shoulder - - | 22 | 10½ |
| From the top of the shoulder, perpendicular height | 10 | 6 |
| From the top of the head, when set up, as he ought to march in state - - - | 12 | 2 |
| From the front of the face to the insertion of the tail - - - - - | 15 | 11 |

Capt. Sandys, of the Bengal establishment, obligingly shewed me a list of about 150 elephants, of which he had the management during the late war with Tippoo Sultaun, in Mysore, and not one of them was ten feet, and only a few males nine feet and a half high. I was very particular in ascertaining the height of the elephants employed at Madras, and with the army under Marquis Cornwallis, where there were both Ceylon and Bengal elephants; and I have been assured, that those of Ceylon were neither higher, nor superior, in any respect, to those of Bengal; and some officers assert, that they were considerably inferior, in point of utility.

The Madras elephants have been said to be from seventeen to twenty feet high; but, to show how much the natives of India are inclined to the marvellous, and how liable Europeans themselves are to mistakes, I will relate a circumstance that happened to myself.

Having heard, from several gentlemen who had been at Dacca, that the Nabob there had an elephant about fourteen feet high, I was desirous to measure him; especially as I had seen him often myself, during the year 1785, and then supposed him to be above twelve feet. After being at Tiperah, and having seen many elephants caught, in the years 1786, 1787, and 1788, and finding all of them much inferior in height to what I supposed the Nabob's elephant, I went to Dacca, in 1789, determined to see this huge animal measured. At first, I sent for the driver,* to ask some questions concerning this elephant; he, without hesitation, assured me he was from ten to twelve cubits, that is, from fifteen to eighteen feet high; but added, he could not, without the Nabob's permission, bring me the elephant to be examined. Permission was accordingly asked, and granted: I had him measured exactly, and was rather surprised to find he did not exceed ten feet in height.

The Honourable Company's standard, for serviceable elephants, is seven feet and upwards, measured at the shoulder, in the same manner as horses are. At the middle of the back, they are considerably higher; the curve or arch of which, particularly in young elephants, makes a difference of several inches.

After an elephant has attained his full growth, it is a sure sign of old age when this curve becomes less; and still more so, when the back is flat, or a little depressed. A partial depression of the spine is, however, no uncommon occurrence, even in very young elephants; and I am convinced it happens from external injury. I have been surprised to see the violence used (in herds of wild elephants just taken) by the large elephants, both male and female, putting the projecting part of the upper jaw, from which the tusks grow out, on the spine of the young ones, and pressing them to the ground, while they roared from pain.

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* Or *Mahote,* as he is generally called.

It has been stated, that the sagacity of the elephant is so great, and his memory so retentive, that when once he has received an injury, or been in bondage, and afterwards escapes, it is not possible, by any art, again to entrap him. Great as my partiality is for this noble animal, whose modes of life and general sagacity I have had so many opportunities of observing, yet a regard to truth compels me to mention some facts, which contradict that opinion. The following history of an elephant taken by Mr. LEEKE,* of Longford Hall, Shropshire, contains many interesting particulars on this subject. The elephant was a female, and was taken at first, with a herd of many others, in the year 1765, by Rajah KISHUN MAUNICK,† who, about six months after, gave her to ABDOOR REZAH, a man of some rank and consequence in the district. In 1767, the Rajah sent a force against this ABDOOR REZAH, for some refractory conduct, who, in his retreat to the hills, turned her loose into the woods, after having used her above two years, as a riding elephant. In January, 1770, she was retaken by the Rajah; but, in April, 1771, she broke loose from her pickets, in a stormy night, and escaped to the hills. On the 25th of Dec. 1782, she was driven by Mr. LEEKE's elephant hunters into a *keddah*;[*] and, the day following, when Mr. LEEKE went to see the herd that had been secured, this elephant was pointed out to him by the hunters, and particularly by a driver who had had charge of her for some time, and well recollected her. They frequently called to her by name; to which she seemed to pay some attention, by immediately looking towards them, when her name, *Juggut-Peāuree*, was repeated; nor did she appear like the wild elephants, which were constantly running about the *keddah* in a rage, but seemed perfectly reconciled to her situation.

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* He was then the Resident of Tiperah, and took some pains to ascertain the facts here mentioned.

† The Rajah is the principal Zemindar in the province of Tiperah, paying the usual revenue for his lands in the low country; but, in the hills he is an independent sovereign, has the power of life and death over his subjects, a mint, and other insignia of sovereignty.

From the 25th of December to the 13th of January, (a space of eighteen days,) she never went near enough the outlet (or *roomee)* to be secured; from a recollection, perhaps, of what she had twice before suffered.[†] Orders, however, had been given, not to permit her to enter the outlet, had she been so inclined, as Mr. LEEKE wished to be present when she was taken out of the *keddah*. On the 13th of January, 1783, Mr. LEEKE went out, when there were only herself, another female, and eight young ones, remaining in the inclosure. After the other female had been secured, by means of the *koomkees*[‡] sent in for that purpose, the hunters were ordered to call *Juggut-Peāuree.* She immediately came to the side of the ditch, within the inclosure; on which, some of the drivers were desired to carry in a plantain tree, the leaves of which she not only took from their hands, with her trunk, but opened her mouth, for them to put a leaf into it, which they did, stroking and caressing her, and calling to her by name. Mr. Leeke, seeing the animal so tame, would not permit the hunters to attempt tying her; but ordered one of the trained elephants to be brought to her, and the driver to take her by the ear, and order her to lie down. At first, she did not like the *koomkee* to go near her, and retired to a distance, seemingly angry; but, when the drivers, who were on foot, called to her, she came immediately, and allowed them to stroke and caress her, as before; and, in a few minutes after, permitted the trained females to be familiar. A driver, from one of these, then fastened a rope round her body, and instantly jumped on her back; which, at the moment, she did not like, but was soon reconciled to it. A small cord was next fastened round her neck, for the driver to put his feet in, who, seating himself on the neck, in the usual manner, drove her about the *keddah*, the same as any of the tame elephants.

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[*] The inclosure in which elephants are secured. *Vide* Asiatic Researches, Vol. III. Art. " Method of catching Elephants."

[†] When elephants were secured in the outlet from the *keddah*, they bruised themselves terribly. *Vide* Asiatic Researches, Vol. III.

[‡] *Koomkees* are female elephants, trained for the purpose of securing wild elephants, and more particularly those large males which stray from the woods, named *goondahs*. *Vide* Asiatic Researches, Vol. III.

After this, he ordered her to lie down, which she instantly did; nor did she rise till she was desired. He fed her from his seat, gave her his stick to hold, which she took with her trunk, and put into her mouth, kept, and then returned it, as she was directed, and as she formerly had been accustomed to do. In short, she was so obedient, that had there been more wild elephants in the *keddah*, to tie, she would have been useful in securing them.

Mr. LEEKE himself then went up, took her by the ear, and bade her lie down; a command she instantly obeyed.

I have known several other instances of elephants being taken a second time; and was myself a witness both of the escape and retaking of one, as related in the following account.

In June, 1787, *Játtra-Mungul*, a male elephant, taken the year before, was travelling, in company with some other elephants, towards Chittigong, laden with a tent and some baggage, for our* accommodation on the journey. Having come upon a tiger's track, which elephants discover readily by the smell, he took fright, and ran off to the woods, in spite of the efforts of his driver. On entering the wood, the driver saved himself, by springing from the elephant, and clinging to the branch of a tree under which he was passing; when the elephant had got rid of his driver, he soon contrived to shake off his load. As soon as he ran away, a trained female was dispatched after him, but could not get up in time to prevent his escape; she, however, brought back his driver, and the load he had thrown off, and we proceeded, without any hope of ever seeing him again.

Eighteen months after this, when a herd of elephants had been taken, and had remained several days in the inclosure, till they were enticed into the outlet, there tied, and led out in the usual manner, one of the drivers, viewing a male elephant very attentively, declared he resembled the one which had run away. This excited the curiosity of every one, to go and look at him; but, when any person came near, the animal struck at him with his trunk, and, in every respect, appeared as wild and outrageous as any of the other elephants.

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* Mr. BULLER and myself.

At length, an old hunter, coming up and examining him narrowly, declared he was the very elephant that had made his escape about eighteen months before.

Confident of this, he boldly rode up to him, on a tame elephant, and ordered him to lie down, pulling him by the ear at the same time. The animal seemed quite taken by surprise, and instantly obeyed the word of command, with as much quickness as the ropes, with which he was tied, permitted; uttering, at the same time, a peculiar shrill squeak through his trunk, as he had formerly been known to do; by which he was immediately recognized, by every person who had ever been acquainted with this peculiarity.

Thus we see that this elephant, for the space of eight or ten days, during which he was in the *keddah*, and even while he was tying in the outlet, appeared equally wild and fierce as the boldest elephant then taken; so that he was not even suspected of having been formerly taken, till he was conducted from the outlet. The moment, however, he was addressed in a commanding tone, the recollection of his former obedience seemed to rush upon him at once; and, without any difficulty, he permitted a driver to be seated on his neck, who, in a few days, made him as tractable as ever.

These, and several other instances which have occurred, clearly evince, that elephants have not the sagacity to avoid a snare into which they have, even more than once, fallen.

The general idea, that tame elephants would not breed, has doubtless prevented trials being made, to ascertain whether, under particular circumstances, this supposed reluctance could be got the better of.

I was however convinced, from observation, as well as from some particular facts, that elephants had their seasons in which they were in heat; I shall, therefore, first mention the circumstances which induced me to attempt breeding from tame elephants, and then relate the success of the experiments instituted for this purpose.

The circumstances to which I allude, happened in January, 1790, at a *keddah* near to Comillah, the capital of Tiperah.

Messrs. HENRY BULLER and GEORGE DOWDESWELL, of Chittigong, being then on a visit at Comillah, accompanied me and several others, to see a herd of elephants which had been lately taken. Our visitors then proposed a trial being made, of tying the wild elephants immediately, in the *keddah*, in the manner practised at Chittigong, instead of waiting till they were enticed, one after another, into the narrow outlet, there to be secured, and led out in the usual manner.*

This mode they recommended so earnestly, from a conviction of its superior utility,† that Mr. JOHN BULLER, to whom the *keddah* belonged, assented to the trial being made, and gave orders for the trained females, and proper assistants, to go directly within the inclosure. Having but few trained females present, it was judged advisable to send in a fine male elephant, taken many years before, and thoroughly broke in, to assist them, as well as to keep the herd in awe. He had no sooner entered the inclosure, and been brought near the herd, than, discovering one of the females to be in heat, impelled by desire, and eager to cover her, he dashed through the herd, regardless of the orders and severe discipline of the driver, and had nearly accomplished his purpose. The driver, being alarmed for his own safety, exerted in vain all his strength, to turn him, and bring him from among the wild elephants; but the drivers of the trained females, coming speedily to his assistance, soon surrounded this furious animal, and separated him from the herd. In resentment, however, of his disappointment, he attacked a small *koomkee*, with such violence as completely overturned her and her rider; and, had he not been of a particular species, called *mucknah*, which have only small tusks, he most probably would have transfixed, and killed her on the spot: fortunately, neither she nor her driver received any considerable hurt. This accident prevented the trial being then made, to tie the wild elephants in the manner proposed.

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* *Vide* Asiatic Researches, Vol. III. article, "Method of catching wild Elephants;" where this process is particularly described.

† Though fully convinced of this, I could not bring the hunters to adopt the Chittigong method, till the year 1794. After this, during the last three years I remained at Tiperah, I did not lose one elephant in twenty; whereas, by the former method, of tying them in the *roomee*, near one-third of those taken died in less than a year, in consequence of the hurts they received from their violent efforts to get free, before they could be properly secured. The natives of Tiperah, and indeed of most parts of India, are extremely attached to old customs; and it was with the utmost difficulty I prevailed on the hunters to deviate from the practice of their ancestors, though the method recommended was followed at Silhet, as well as at Chittigong. The method was, simply to surround a herd, in the first convenient place, with a ditch and palisade; and, when this was finished, to send in the *koomkees*, and proper persons to tie the wild elephants on the spot, and then conduct them, one by one, through an opening in the palisade, from the *keddah*, as soon as they were tied.

Reflecting on the disobedience shown by an elephant remarkably docile, and which had been domesticated for many years, when his passions were excited, and recollecting also, that a wild elephant had covered a female, in February, 1778, before many spectators, just after the herd had been secured in the inclosure, I was assured in my own mind, that it was not from any sense of modesty, either wild or tame elephants did not gratify their passions in public; but no opportunity offered of prosecuting this inquiry, till 1792. Having then taken upon myself the management of the elephant hunters, a very fine male was caught in November: he was both young and handsome, and also of a most docile disposition; I therefore promised his driver a considerable gratuity, if he would get him into high order, so that I might have an opportunity of bringing his procreative powers to trial, with a tame female.

In the month of March, 1793, the driver of a favourite female elephant informed me, that she had then signs of being in heat; and that, if the male and she were kept together, and highly fed, an intimacy would probably soon take place. They were therefore, shortly after this, brought near to Comillah, where a spacious shed was erected for their accommodation.

In the day, they went out together, to feed; they also brought home a load of such succulent food as their drivers and attendants could collect. After their return, they stood together, slept* near each other, and every opportunity was granted them to form a mutual attachment. In the evening, they had each from ten to twelve pounds of rice soaked in water, to which a little salt was added; and, from the middle of May till the latter end of June, some warm stimulants, such as onions, garlic, turmeric, and ginger, were added to their usual allowance of rice. Long before this, however, a partiality had taken place, as was evident from their mutual endearments, and caressing each other with their trunks; and this without ceremony, before a number of other elephants, as well as their attendants.

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* It is always a good sign, when an elephant lies down to sleep, within a few months after he is taken; as it shews him to be of a good temper, not suspicious, but reconciled to his fate. Elephants, particularly *goondahs*, have been known to stand twelve months at their pickets, without lying down to sleep; though they sometimes take a short nap standing.

Near the end of June, I was satisfied the male would not, even to regain his freedom, quit the object of his regard; I therefore ordered the keepers to picket the female, by one of her fore-legs only, in the house where they stood, but to leave the male at full liberty. Fearful, however, of hurting their supposed delicacy, and thinking the nearness and sight of the attendants might possibly give umbrage to their modesty, I desired them to remain quiet in a little hut, erected on the outside of the building appropriated to the elephants, where they could see equally well as if nearer.

On the evening of the 28th of June, 1793, the male was let loose from his pickets; and, soon after, he covered the female without any difficulty, although before this she never could have received the male, being taken when very young, about five years and a half prior to this period. The male was then led quietly to his stall; but, early on the morning of the 29th, he became so troublesome, that the drivers, in order, as they said, to quiet him, but partly, I suspect, to indulge their own curiosity, permitted him to cover her a second time; which he readily did, before the usual attendants, as well as a number of other spectators. After this, the driver brought me a particular account of the whole process. Though much pleased with the success of the experiment, yet I was rather chagrined he had not given me notice, that I might have been myself an eye witness; and therefore told him, he should not receive the promised reward, till I had satisfied myself of the fact.

About two in the afternoon of the same day, I was desired to repair to the place where the elephants stood, as the male had been trying to get nearer the female. On this, I proceeded to the spot, with my friend Captain ROBERT BURKE GREGORY: when we arrived, I ordered the male to be freed from his shackles; and, after some toying, and a few mutual caresses, we had the satisfaction to see him cover the female.

When the male mounted, he placed one of his fore-legs on each side of her spine, with his feet turned to, and pressing against, her shoulders, and his trunk over her forehead; supporting himself firmly in this situation, during coition, which he continued nearly the same time, and in the same manner, as a horse with a mare.

The female remained perfectly still, during the coitus. When the male had finished, he stood quietly by her side, while she caressed him with her trunk; and, as they then appeared well pleased, and gentle as usual, I went up and patted them both, as I had formerly been accustomed to do, without the smallest apprehension. In the evening, they were brought home to be fed; and, though only a few hours had elapsed since his last embrace, the male seemed inclined to make another attempt; to which I would have consented, to gratify a crowd of people then present, had I not now learned, that he had covered the female in the open plain, about ten in the morning, when going out for food, in spite of the exertions of the drivers and attendants; at least so they alleged, in excuse for having permitted it, contrary to my orders. As he had already covered four times in about sixteen hours, I was afraid a further indulgence might be prejudicial, and therefore would not permit it; especially as Mr. IMHOFF, to whom he then belonged, was absent. That gentleman, however, returned two days after; but, when the two elephants were brought together, in order that Mr. IMHOFF's curiosity might be indulged with so novel a sight, the female, being no longer in heat, was so uncivil as to give the male a kick in the face, when he was using what she then thought improper liberties; nor did she afterwards permit him to cover her, though, when standing together, they mutually indulged in a few caresses.

During the time they were kept together, the male never showed signs of his passions being excited, by any exudation from the ducts of the glands near his temples; which is generally considered as the sign of a male elephant being peculiarly ready for the female. This, however, I am inclined to believe is a vulgar error; as not one of the male elephants I have seen cover, in a domestic state, nor any of the males which were caught singly, or rather entrapped, by their desire to have connexion with the tame females, had, at those times, the smallest appearance of such an exudation. Had this happened, in any one instance, during my residence in Tiperah, I think I must have known it; for, when this exudation takes place, the elephant has a dull heavy look, and it is dangerous for strangers to go near him. I have seen elephants in this situation, after they had been many years caught; but, though they were then said to have their passions excited, I have never known one to cover during the continuance of this exudation: nor have elephants, so far as I have been able to observe, any particular seasons of love, like horses and cattle. Of five instances of elephants covered at Tiperah, one received the male in February, another in April, a third in June, a fourth in September, and the fifth in October. Besides these, an attempt was made by a tame male, to cover, in the month of January, a wild female, then in heat.* When the female is in heat, the parts of generation show it, by an unusual fulness of the labia; and, if she is placed near a male, she endeavours, by caresses, to excite his desires.*

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* Many pregnant females are taken every year at Tiperah, and produce young ones in the different months: this clearly shows, that there are no particular seasons during which the females are in heat.

After the female had been covered by the male, as has been just related, there being then no other female ready, he was placed with an elephant which had had a young one about four years before this, and some months ago was reported to have been in heat. It was thought, after some trial, that she was likely to permit him to cover, as she caressed him occasionally, and roused his passions; but she would not allow him to gratify his desire.

The drivers, tired of this coyness, and stimulated perhaps by the hopes of another gratuity, were so brutal as to tie her, and let the male make an attempt upon her, while tied. His attempt, however, was to no purpose; though he continued his efforts till he appeared to be quite exhausted. This being told me, I severely reprimanded the people; and ordered the female to be left at full liberty to reject or receive the male, as she might think proper.

Here, however, was positive proof, that the male would have effected his purpose by force, when he found he could not obtain it any other way. He remained at Comillah till October, 1793, without my being able to procure a female that was in heat; he was then sent to Calcutta.

I now became extremely solicitous about the health of the female which was covered in June; and gave particular directions not to overheat her, but merely to give her as much food and exercise as were likely to keep her in the best condition, as she was now known to be pregnant. In three months after she was covered, she became fuller, her flesh felt softer, and her breasts began to swell. These marks of her being with young, were so evident to the driver, that he mentioned them of his own accord; which convinced me, that an elephant, three months after conception, may be known by the keepers to be pregnant.

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* It may be proper to observe, that the penis of a full grown elephant is from two feet four to two feet six inches in length, and from fourteen to sixteen inches in circumference. I caused the penis of two males to be measured, after their passions were excited, in order to ascertain the real size. On some occasions, I have seen the penis absolutely touch the ground, when the elephant has been walking; but it must be recollected, that the hind legs of an elephant are very short, in proportion to his size.

She had always been a favourite, from having been the gift of my worthy and respected friend Mr. JOHN BULLER,* as well as from her gentle and docile disposition; and I now had hopes of her going her full time.

She was seven feet three inches high, when covered; but, after this, increased so fast, not in bulk only, but also in height, as to exceed seven feet eight inches, before she brought forth. On the 16th of March, 1795, she produced a fine male; just twenty months and eighteen days after she was first covered.

The young one was thirty-five inches and a half high; and had every appearance of having arrived at its full time, being the largest I had known produced in Tiperah.

We have many young produced every year, by the females which are taken while breeding, and these seldom exceed thirty-four inches; this, however, may be owing to the weak and reduced state the mothers are brought to, while breaking in.

The young of the elephant, at least all those I have seen, begin to nibble and suck the breast soon after birth; pressing it with the trunk, which, by natural instinct, they know will make the milk flow more readily into the mouth, while sucking. Elephants never lie down to give their young ones suck; and it often happens, when the dam is tall, that she is obliged, for some time, to bend her body towards her young, to enable him to reach the nipple with his mouth; consequently, if ever the trunk was used to lay hold of the nipple, it would be at this period, when he is making laborious efforts to reach it with his mouth, but which he could always easily do with his trunk, if it answered the purpose. In sucking, the young elephant always grasps the nipple (which projects horizontally from the breast) with the side of his mouth.

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* Now one of the Members of the Board of Revenue, at Calcutta.

I have very often observed this; and so sensible are the attendants of it, that, with them, it is a common practice to raise a small mound of earth, about six or eight inches high, for the young one to stand on, and thus save the mother the trouble of bending her body every time she gives suck, which she cannot readily do when tied to her picket.

Tame elephants are never suffered to remain loose; as instances occur of the mother leaving even her young, and escaping into the woods.

Another circumstance deserves notice: if a wild elephant happens to be separated from her young, for only two days, though giving suck, she never afterwards recognizes or acknowledges it. This separation sometimes happened unavoidably, when they were enticed separately into the outlet of the *keddah*. I have been much mortified at such unnatural conduct in the mother; particularly when it was evident the young elephant knew its dam, and, by its plaintive cries and submissive approaches, solicited her assistance.

Here it may be observed, that a female was believed to have gone twenty-one months and three days; being supposed to have been covered on the 13th of January, 1788, some days before she was driven into the inclosure. When I made particular inquiry as to the real time she was taken, the superintendant* of the hunters said it was in January; but the principal hunters† declared she was among the herd taken in February following, and was probably the same elephant Mr. Buller, Captain Hawkins, and many others, saw covered on the 9th and 10th of that month. Perhaps, some days prior to this, she might have been covered in the woods, before she was brought into the inclosure; but, as a herd was taken in each of those months, and not kept separate, and two years had nearly elapsed before I thought of making any inquiry, it was impossible for me to determine in which of those months she was really taken; and the only motive I then had for endeavouring to ascertain this point, was to form some probable conjecture as to the period of an elephant's gestation, which has now been ascertained, in the instance before related.

Early in September, 1795, the female that had been covered, and had bred under my own observation, was known to be in heat; this was less than six months after bringing forth. Learning, at the same time, that the Rajah of Cudwah, a principal Zemindar of the province, had a very large male that had been in the family near twenty years, from the time he was about five years old, I sent a messenger, requesting the elephant might be sent to Comillah, which request the Rajah immediately complied with.

To prevent any interruption from the number of spectators, the elephants were put into a small inclosure, on the 17th of September; the female was picketed by one leg, and the young one, to which she was giving suck, was tied to a tree at some distance, fearing, if permitted to run about, he might receive some injury.

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* The *Dârogah.*

† The *Dydars.*

After a few caresses from the female, the male at length effected his purpose, and covered her twice the same evening. As the intention of the male elephant's visit was known in the district, and a few days had elapsed since the two elephants were brought together, in order to make them acquainted, the number of spectators was greater than on any other similar occasion.

She was afterwards covered, several times, on the 20th of the same month; the male, in this case, being admitted after an interval of three days, although formerly, in June, 1793, she refused him when only two had elapsed. She again proved with young; and, in November, 1796, being myself in a bad state of health, and under the necessity of returning to Europe, I sent her to Lucknow, together with her young one, at the request of my friend Captain David Lumsden: though she was then very big, she was still giving suck.

About a month before that period, I got my friend, Mr. Stephen Harris, to permit a female of his to be covered; the same which had, in 1793, rejected the attempts of the male to cover her contrary to her inclination. Another messenger was dispatched to Cudwah, for the Rajah's elephant, which was again sent to Comillah. He covered her repeatedly, on the 14th, 15th, and 16th of October, 1796, before many Europeans, as well as natives; and, the last time he covered her, it was evidently contrary to her inclination; so that, in fact, he used force to effect his purpose, and held her so firmly, that the marks of the nails of his fore-feet were deeply imprinted on her shoulders.

Having mentioned a sufficient number of instances, to prove the ability, as well as the inclination of the elephant, to propagate his species in a domestic state, and that without any signs of modesty, and having ascertained the period of gestation to be twenty months and eighteen days, it may be necessary to observe, that it is a difficult matter to bring a male, which has been taken about the prime of life, into good condition to act as a stallion; for, being naturally bolder, and of a more ungovernable disposition, than the female, he is not in general easily tamed, till reduced very low; and it requires considerable time, as well as much expence and attention, before he can be brought into such high order as is requisite. He must also be of a gentle temper, and disposed to put confidence in his keeper; for he will not readily have connexion with a female, whilst under the influence of fear or distrust. Of this I have seen many instances; nor do I recollect one male elephant in ten, which had been taken after having attained his full growth, much disposed to have connexion with a female. This is a most convincing proof, that those males which are taken early in life, and have been domesticated for many years, more readily procreate their species than elephants taken at a later period. In their wild state, however, they show no reluctance; for, besides all the males that are entrapped, from their desire to have connexion with the trained females which, though not in heat, are carried out to seduce them, several instances have occurred, of wild elephants covering, immediately after being taken, in the *keddah.*

On the 3d of April, 1795, a very fine male elephant covered a female twice, in the midst of the herd, and before all the hunters. On the 4th, I saw him attempting to cover a third time, when he was suddenly disturbed, by the noise the hunters made to drive away some of the herd which had come too near the palisade. In consequence of this interruption, he threw down first one and then another small elephant, and gored them terribly with his tusks, though they came between him and the female only for their protection: he had, before this, killed four, and wounded many others. When the poor animals were thrown down, conscious of their impending fate, they roared most piteously; but, notwithstanding their prostrate situation, and submissive cries, he unfeelingly and deliberately drove his tusks through, and transfixed them to the ground; yet none of the large elephants, not even the dams of the sufferers, came near to relieve them, or seemed to be sensibly affected. This savage animal had been then confined four days in the inclosure, along with the herd, upon a very scanty allowance of food, and could have but very little hope of escaping; yet here his passions were stronger than his fears. It was on account of this savage disposition, the hunters had asked permission to shoot him, before I had either seen him or the herd, and thence judged he was a *goondah*,* that had lately joined. Having never before known any elephant killed wilfully, in the *keddah*, by the larger males, and having no idea that he would commit such terrible havock, I unluckily refused to grant their request, being desirous to save so stately an elephant. When the palisade was finished, I got him tied, and led out; but, not brooking restraint, he languished about forty days, after he was secured, and then died.

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* From this instance, as well as many concurring circumstances, I am convinced that these *goondahs* generally leave the herd of their own accord, and join it when they think proper, or are induced to it from a female being in heat; yet it has been supposed, that they are driven from the herd, at an early period of life, by their seniors. This appears improbable, as it is not often that very large males are taken with a herd of elephants; for, depending on their own strength, they stray singly, or in small parties, from the woods into the plains, and even to the villages; and it is in these excursions they are taken, by means of the trained females. As these *goondahs*

In the course of this narrative, I have, in general, related only such particulars concerning the elephant as came within my own knowledge, and which were either not known, or not published. To enter into a particular history of the elephant was not my intention; and, although the procreation of tame elephants has been proved, yet the expence incurred by breeding them, may deter others from making attempts of this kind. But it opens a field of curious inquiry to the naturalist; and, now that the facility with which it may be done is ascertained, it suggests itself as a mode by which the breed of elephants may be improved, in size, strength, and activity. In this way, any expence which might be incurred, would more than repay itself, in the future benefits to be derived from a superior breed of elephants.

are much larger, and stronger, than the males generally taken with the herd, it is not probable they would submit to be driven from it, unless at an early period. I have seldom seen, in a herd of elephants, a male so large as may be commonly met with among two or three *goondahs*; but, if these last were driven from the herd when young, the very reverse would be observed.

# VI. *On the Decomposition of the Acid of Borax or Sedative Salt.*

*By* Lawrence de Crell, *M. D. F. R. S. Lond. and Edinb. and M. R. I. A. Translated from the German.*

Read February 7, 1799.

The salt called Borax, so useful in various manufactures and arts, and hitherto imported only from Thibet and Persia, or, in small quantities, from Tranquebar,* has ever excited the attention of natural philosophers. This attention was principally directed to the acid (called sedative salt) contained in it; its other component part, the alkaline salt, (soda or natron,) being better known, and found in many other natural productions, either alone, or in conjunction with other acids. The acid abovementioned has hitherto been discovered only by Höfer, in the lagone of Castelnuovo; by Martinovich, in the petroleum of Gallicia,† mixed with alkaline earth; and by Mr. Westrumb, near Luneburg. The scarcity of this acid, and its being found only in the substances and situations abovementioned, occasioned a supposition, in the minds of those who minutely observe and examine the course of nature, that it is not a simple substance, but is formed afresh from a variety of other substances, previously decomposed, by a singular coincidence of operative causes; and, consequently, that it belongs to compounds.

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* Demachy's *Laborant im Grossen,* Part 2, page 89.

† Crell's *Annalen,* 1791. T. I. p. 162.

Numerous have been the experiments made by chemists, who supposed they had formed this salt by composition: some described experiments, which they declared to have succeeded with them, though they always failed, when attempted by others;[*] from which, LEONHARDI concludes, that nothing more can be expected from any similar attempts to produce sedative salt.[†]

I was surprised that these chemists had never (so far as I knew) examined the subject by the way of analysis, and endeavoured to decompose the sedative salt already formed by nature. Indeed, no great hopes of success could be entertained, as daily experience shows, that though this salt be kept fluid, in the hottest fire, for many hours together, till it becomes a vitrified substance, yet, when it is afterwards dissolved in distilled water, the solution is complete, without any residuum, and it then shoots into crystals of precisely the same salt as before. Notwithstanding all this, when I reflected, that borax is generated only in certain climates of the East, and that its acid is found only in particular substances and situations, as has been already mentioned, I could not but suppose the latter to be the produce of a new formation. This being premised, I considered maturely in what manner the decomposition of this new and extraordinary compound might be attempted. Admitting the composition to be formed by the coalition of a number of different substances, it seemed not improbable but that an acid, penetrating into and dissolving the whole mass, would rather associate with some than with others of its various component parts, and thus produce a separation or change of the latter. Besides, as the sedative salt, strong as its operation is (in a high degree of heat) upon almost all neutral salts, has but a faint taste of acid, it might be supposed, that its acid is contained within some unknown species of earth, intimately combined; or within some sort of inflammable matter; or, according to a phrase used in the new system, there might be a deficiency of acid matter; that, therefore, some more powerful acid would probably separate and dissolve the earthy particles, destroy or change the inflammable matter, or impart the acid it might be supposed to want.

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[*] See FUCHS *Geschichte des Boraxis.*

[†] MACQUER's Dictionary, translated by LEONHARDI, 2d edit. Vol. V. p. 588.

My choice, among the different acids, was fixed upon that particular one which, though not always quick in its operation, never fails to penetrate deep into all soluble substances, is nearly related to all inflammable bodies, and possesses an abundance of acid matter; I mean the oxygenated muriatic acid, prepared with manganese. In the application of this menstruum, I resolved to follow the practice established by the constant experience of both ancient and modern chemists; which has taught us, that difficult decompositions of parts closely united, are more easily effected by a gentle, long continued, digestive heat, and repeated distillation of the same menstruum, than by a heat which is more violent, and operates more quickly.

I first made some preliminary experiments, in order to judge what probability there might be of success.

*Experiment* 1. I poured an ounce and a half of the above-mentioned acid upon two drams of sedative salt, in a retort, to which I adapted a proper receiver, and then placed the mixture in a gentle digestive heat, of from 140° to 200° of FAHRENHEIT. The fluid was distilled over very slowly, and the salt was dry on the third day. The salt in the retort seemed unchanged: nor had the marine acid lost any thing of its usual smell.

*Exp.* II. I poured the distilled fluid out of the receiver upon the same salt, and exposed them to the same degree of heat as before. The salt again became dry on the third day, but there was yet no appearance of any change.

*Exp.* III. I repeated the same process a third time. I now perceived, during the distillatory digestion, several bright yellow spots upon the salt, as it ascended the sides of the retort, resembling well-formed ammoniacal flowers of iron; more of which I discovered, after the entire exhalation of the fluid.

*Exp.* IV. The above change induced me to repeat the distillation; and I then perceived, not only as many, but a much greater number of bright yellow spots, some of which were even much darker in colour, and approaching to brown. A change had now evidently taken place, which change increased upon every repetition of the process; I therefore judged I might follow this direction with confidence. But, with a view to use the greatest accuracy and precaution in my proceedings and observations, I resolved to begin my work over again.

First, I procured some ounces of sedative salt, which had been obtained from borax by means of vitriolic acid; and then prepared two quarts of the abovementioned oxygenated muriatic acid, by distilling three parts of muriatic acid with one part of the purest manganese, in the usual manner; this I preserved in a cool dark place. Thus, the substances used in the following experiments, were always of the same nature.

*Exp.* V. I poured three ounces of the oxygenated muriatic acid upon half an ounce of the sedative salt, in a white glass tubulated retort. I used such a retort, that (in frequently pouring back the distilled fluid) I might not have to lute afresh the several vessels, after every distillatory digestion. For the same reason also, I chose a tubulated receiver, the tube of which gradually terminated in a point, in shape of a funnel. This tube passed into a phial, placed in such a manner that all the fluid passing into the receiver dropped immediately into the phial, the joinings of which were closed with bladder. To close the tube of the retort, I did not think it right to use a waxed cork, (though it closes very tight,) because it might be corroded; and also, because the vapours, dropping from the cork, might carry some fat and oily matter back into the retort. For the same reason, I would not use any greasy lute; but closed the joints of the glass stopper (which fitted remarkably close) with a ring of fine sealing-wax, closely pressed upon it, but which could easily be disengaged, after my work was done, while the retort was still warm: and, as I was even afraid of an oily lute about the joints of the receiver, I closed them up with a ring of very fine white clay, which I fitted to them as exactly as possible, by pressure; letting it stand several days, to dry, and then carefully filling up all the cracks. Having made this previous arrangement, and put the abovementioned ingredients together, I suffered them to remain cold for twenty-four hours; at the end of which, the salt was not entirely dissolved, but, upon the application of heat, the whole became a clear fluid.*

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* This appeared to me so striking, that I endeavoured to obtain a confirmation of it. I made a similar mixture, in the same proportions, which was not dissolved so long as it remained cold; but was dissolved by heat. When the solution cooled, a

The degree of heat in the sand was from 180° to 240°, by which the fluid evaporated very slowly. During this operation, there ascended, or rather crept up the sides of the retort, a considerable quantity of salt, in very loose flowers, rising pretty high above the fluid, increasing by degrees, and chiefly occupying that half of the retort which received a greater degree of heat than the other; but never the opposite or colder half. In four days, the fire being extinguished towards the evening of the last, the fluid had evaporated, so as to leave the salt apparently dry. After cooling for some time, the bladder upon the phial was moistened by water, and the vessels were separated; the sealing-wax also having been removed, and the stopper taken out, the distilled fluid was poured back, through a glass funnel, upon the salt, without disturbing the lute.

*Exp.* VI. As soon as the fluid was added, the salt at the bottom began by degrees to dissolve: that on the sides of the retort did the same, after it was heated, but soon began to form again: the solution appeared of a yellowish hue. In general, however, the whole experiment took the same course as in *Exp.* V. and the smell, both of the salt and the fluid, seemed to be unchanged. The only difference was, that the former did not appear like salt, (the crystallization on the sides excepted,) and in single detached crystals, but something like a white, uniform, spongy, and, as it were, earthy mass. The fluid was now again taken from the phial, as in *Exp.* V. and poured back upon the salt.

*Exp.* VII. VIII. and IX. During the third distillation, bright

small part of the salt (and a larger as the cold increased) precipitated, which was dissolved again, by a fresh application of heat. But, with the degree of heat I employed, no more than one part of salt would dissolve in six parts of the acid.

yellow spots began to appear upon the white flowers; and, after the salt at the bottom had become dry, similar spots appeared upon it, particularly upon the lower surface. The fluid was again, for the fourth time, poured upon the salt, and distilled; when the yellow spots and flowers increased in number. This was also the case in the fifth distillation.

*Exp.* X. The fluid obtained by the last experiment, which had changed a little in smell, and had acquired a particular scent, almost as if some sebacic acid had combined with the muriatic, was poured upon the salt, as before. The number of yellow spots, which had also become of a darker hue, was considerably increased. The salt had now been exposed, ever since the vth. *Exp.* for thirty-two days, to the digestive distillation; and the intermediate time between each distillation, had been longer or shorter, in proportion to the degree of heat, and to the time of kindling and extinguishing the fire. As I now found, that business of importance would prevent me from continuing my labours for some months, I poured two other ounces of the muriatic acid upon the salt, besides the fluid so often drawn off by distillation, and left the mixture at rest.

*Exp.* XI. XII. XIII. XIV. When my business was finished, I again undertook the distilling of the mixture, which had been so long digesting in the cold, for the seventh time, and obtained the same results as in *Exp.* X. Nor was there much difference observed in the XIIth. XIIIth. and XIVth. *Experiments.*

*Exp.* XV. I now poured the fluid obtained by the XIVth. *Exp.* upon the salt, (which had acquired more and more yellow spots, brighter in hue,) and then proceeded as before, till the salt became dry; upon which, when the retort was cool, I poured one ounce and three drams of the muriatic acid, in addition, and allowed the mixture to digest gently for some days.

*Exp.* XVI. In this twelfth distillation, there appeared a large quantity of flocculent sublimate, looking almost like branches, hanging down, and in many places of a yellow colour; it extended even into the neck of the retort, and almost covered the interior aperture of the tube.

*Exp.* XVII. The thirteenth distillation produced the same phænomena. Upon the lowermost surface of the mass of salt, many light-brown spots appeared, as soon as the fluid was so much evaporated that no more of it could be seen upon the salt.

From all these circumstances, I now believed the mass of salt, by a digestion of twenty-two days, and seven distillations, from *Exp.* XI. to XVII. (that is, by a digestion of fifty-four days, and thirteen distillations, in the whole,) to be so far decomposed, as to admit of a separation of some of its constituent parts. I therefore supposed I might leave off applying only a digestive warmth, and proceed to a greater degree of heat.

*Exp.* XVIII. Having poured out the fluid obtained by *Exp.* XVII. and replaced the phial, I increased the degree of heat. By this, the retort became quite obscured, first by fumes, and afterwards by a quantity of white sublimate, attaching itself to all its sides, which, however, had not the appearance of common sedative salt. As I increased the heat, the sublimate grew darker in colour; afterwards became black and frothy; and, at length, ran down the sides of the retort, in different places, like thick oil of hartshorn, the retort being almost wholly blackened by it.

*Exp.* XIX. While the retort was still warm, I poured into it the fluid obtained by *Exp.* XVII. having first warmed it a little; when, almost in the same instant, a very agreeable phænomenon took place. Crystals, perfectly white, shot forth suddenly, and all at once, from every part of the black mass covering the sides of the retort. The distillation being continued, these crystals were at length dissolved, and entirely removed. The supernatant fluid was, as usual, almost colourless. When the mass of salt appeared dry, the fire was increased, as in *Exp.* XVIII. and the same appearances as above related took place: first, the sublimate appeared white, then black, frothy, and flowing down the sides.

*Exp.* XX. I proceeded, as in *Exp.* XIX. to pour back the distilled fluid. Instantly a number of the whitest crystals shot forth from the black ground, forming small groups; but the retort was cracked.

*Exp.* XXI. I therefore took all the vessels asunder, and shook the retort well, till whatever hung upon its sides was dissolved; then distilled the fluid in another retort, till the mass of salt appeared quite dry. I now put the retort into a crucible, surrounded it with sand, fitted another receiver to it, and placed the crucible in an open fire. First, some sublimate was produced, towards the neck of the retort, (but which vanished as the heat increased,) and then a small portion of fluid, (hardly more than a dram, or a dram and a half,) which appeared to smell a little of the sebacic acid. At the bottom of the retort was a blackish mass, *a*, and likewise some sublimate, *b*, which, by its varied appearance, seemed to be of a two-fold nature.

*Exp.* XXII. The residuum taken out of the broken retort had a spongy appearance, and swam upon water; it had a blackish colour, and weighed three drams and ten grains.

Being exposed to the air, the blackish colour became lighter, and inclining to grey. When digested in sixteen parts of distilled water, in the usual temperature, for two days and a half, it did not all sink to the bottom; and, after being digested with heat for twenty hours, it was not entirely dissolved: that part which sank, was of a blackish brown. More water was then added, and it was made to boil for two hours; it was afterwards placed upon a paper filter, (the weight of which was previously ascertained,) and edulcorated with boiling distilled water, till at last a proportion of twenty-six parts of water to the substance had been used. After all the fluid, α, had passed through, and the filter, with the residuum, had been dried in a heat of 212°, for an hour and a half, the residuum, β, weighed, exclusive of the filter, nineteen grains.

*Exp.* XXIII. The fluid, α, obtained by *Exp.* XXI. was suffered to evaporate gradually, and yielded three drams and ten grains of a white transparent salt.

*Exp.* XXIV. This salt (obtained by *Exp.* XXIII.) was put into a small retort, and exposed, in a crucible filled with sand, to an open fire. It became of a blackish-brown colour, yielded some sublimate, *a*, (about five grains,) a small portion of fluid, *b*, and a blackish-brown residuum, *c*, which grew lighter in colour, on being exposed to the air.

*Exp.* XXV. The fluid, *b*, (of *Exp.* XXIV.) smelt like marine acid, and precipitated nitrate of lead.

*Exp.* XXVI. The residuum, *c*, (of *Exp.* XXIV.) by the addition of some water, became whiter, and was dissolved; more water having been added, it was digested with heat, by which the matter was dissolved. The solution being afterwards filtered, I obtained two drams and four grains of white salt: the residuum upon the filter weighed four grains.

*Exp.* xxvii. This salt (*Exp.* xxvi.) I again exposed to the fire; when it yielded from twenty to thirty drops of acid liquor, four grains of sublimate, and a residuum, which, being dissolved, yielded one dram and thirty-three grains of salt, and left two grains and a half, *c*, upon the filter.

The same salt, (obtained by *Exp.* xxvi.) being distilled, became of a brownish-grey colour; and, besides a few drops of fluid, yielded not quite two grains of sublimate. On treating the residuum with water, it yielded sixty-eight grains of salt, and there were not quite two grains left upon the filter.

*Exp.* xxviii. On treating these sixty-eight grains of salt in the same manner, they yielded a few drops of fluid, and two grains of sublimate: after filtration, there remained forty-eight grains of salt, and a residuum of hardly one grain and a half.

*Exp.* xxix. The same salt, treated in the same manner, yielded a few drops, and a little sublimate; and, after filtration, thirty-five grains of salt, and a residuum of hardly one grain.

*Exp.* xxx. On treating these thirty-five grains of salt in the same way, they yielded, besides a very small quantity of fluid and of sublimate, twenty-four grains of salt, and about three quarters of a grain of residuum.

As I now discovered that the quantity of salt was continually decreasing, and some coal separating from it, I thought it superfluous to endeavour to decompose the above twenty-four grains any farther.

*Exp.* xxxi. The residuum, *β*, of *Exp.* xxii. was light, blackish, and like coal. I now poured common concentrated muriatic acid upon three grains of it, and digested the mixture, for forty-two hours, in a considerable degree of heat, but no dissolution was apparent. I then added smoking nitrous acid, and digested it for twenty-four hours, till it boiled, without any apparent dissolution. I added some sugar, (about two grains,) but without effect, except that its colour grew yellowish. I now boiled the fluid, till it all evaporated in reddish-yellow vapours: there remained a very black, thick, glutinous mass, smelling like burnt sugar. Having added three ounces of water, the greatest part of the blackish matter rose to the surface, and the water appeared only a little tinged. The fluid part, indeed, became brown by boiling; but, after rest and subsidence, it again grew clear. I filtered it, *a;* then poured two ounces more distilled water upon the residuum, and, after digesting, boiling, and filtering, added the filtered fluid, *b*, to the former, *a*. After this treatment, there remained two grains of residuum, *c*.

*Exp.* XXXII. Having caused the fluid *a*, *b*, of *Exp.* XXXI. to evaporate, it yielded a salt greyish-yellow mass, which very quickly attracted the moisture of the air. Being again dissolved in water, and saturated with potash, a considerable quantity of whitish earth was precipitated, very much resembling talc.

*Exp.* XXXIII. The residuum, *c*, of *Exp.* XXXI. which, besides its insolubility and lightness, had much of the external appearance of coal, was now thrown upon melted nitre, and it deflagrated. I placed a second crucible with melted nitre close to it, and having, at the same moment, thrown into one the abovementioned residuum, and into the other a quantity of common charcoal pulverized, I could not observe the smallest difference in effect. Very little difference was also apparent, as to the residuum, *β*. of *Exp*. XXII. *c*, of *Exp*. XXIV. and that of the following experiments.

*Exp*. XXXIV. To obviate the objection, that sedative salt alone would perhaps deflagrate with melted nitre, I made that experiment also, but in vain. Not the smallest deflagration took place, even when both were melted together for many hours.

*Exp*. XXXV. Another objection may be made, namely, that in distilling the muriatic acid from manganese, part of the latter had passed over with the acid; and, in the frequent distillations of the sedative salt, had been deposited upon it, and thus deflagrated. But, on throwing fresh pulverized or solid manganese, either such as is usually sold, or quite pure, heated to redness, into melted nitre, not the smallest deflagration took place.

*Exp*. XXXVI. to L. Instead of the interrupted heat used in the foregoing experiments, I now exposed half an ounce of the salt, with three ounces of the oxygenated muriatic acid, to a continued heat, of between 200° and 300° of FAHRENHEIT. The fluid had nearly evaporated in twenty-four hours. I changed the phial, towards the close of the operation, for another, that the former might be gently heated, and the fluid by that means be poured back, with the greater safety, upon the warm salt, through the tube of the retort. In this manner, during an uninterrupted fire of fourteen days, the acid was fourteen times distilled, and returned upon the salt. On the third day, yellow spots appeared. On the fourth, some particles of oil or fat were discovered, swimming on the surface of the fluid in the phial; which particles, after cooling and emptying the phial, adhered to its sides, so as to obscure its transparency. More or less of these oily particles were discovered in every successive operation; and the oily matter, adhering to the inside of the glass, increased considerably.

*Exp.* LI. When the fluid was distilled, the receiver was changed, and the fire increased. A considerable quantity of sublimate was obtained, pretty white in colour, as was likewise the surface of the mass of salt at the bottom of the retort; but, lower down, it was almost of a light ash-grey. After the sublimate ceased to arise, I diminished the fire.

*Exp.* LII. Upon the mass of the former experiment, I poured the fluid obtained by *Exp.* XLIX. and continued a gentle digestion. I very soon perceived something rising towards the surface, and swimming upon it: after some hours, it appeared to be a thick wrinkled skin, like fat, or a skin of mould, increasing in size, until it covered the whole surface. White spots of sublimate appeared upon it, but it did not sink. It assumed gradually a fine lemon colour; and some yellow matter, though not in large quantity, ascended the sides of the retort. The fluid having been gently distilled, and the receiver changed, I placed the retort in an open fire; on which, more sublimate soon appeared; but, not long after, it all vanished, and the retort lost its transparency. The mass contained in it began to rise, first gently, and then violently, especially in the centre, in large frothy bubbles. The distillation was finished, after obtaining one dram of fluid, and when the frothy bubbling had ceased. The retort being broken, that part where the bubbling had been strongest, was found to be black; the upper surface being covered with a thin greyish matter, under which a solid, compact, and almost vitrified substance appeared. Upon this I poured water, and dissolved it in the usual manner; filtered it, let it evaporate, and treated it as described above, *Exp.* XXII.—XXX.

*Exp.* LIII. I obtained a white salt, *a*, and some coal, *b*, (which deflagrated briskly with nitre,) in nearly the same proportions as throughout the series of experiments described from *Exp.* XXII. to XXXIII. which I will not repeat, on account of the little variety observed in them; one of them, however, deserves to be distinguished from the rest.

*Exp.* LIV. I put six grains of the coal, *b*, (of *Exp.* LIII.) in three drams of common muriatic acid, and digested them for two days, till the acid had gradually evaporated. I then added one dram of the same acid, with one scruple of nitric acid, and, when they had evaporated, boiled the residuum full half an hour in distilled water. By this process, I obtained a red solution; and, having saturated it with mild alkali, a sort of skin rose to the surface, with some small pieces of a fat slippery substance, *a*. A considerable quantity of loose earth, *b*, was also precipitated, of a light brown colour.

*Exp.* LV. On throwing the floating pieces, *a*, (*Exp.* LIV.) into a solution of caustic alkali, they dissolved; the solution had a reddish-brown colour.

*Exp.* LVI. With the same solution of caustic alkali, I covered the light brown earth of *Exp.* LIV. As the solution changed its colour to a reddish brown, the earth gradually became perfectly white.

*Exp.* LVII. To observe the affinity of other acids to the sedative salt, I poured six drams of nitrous acid upon two drams of the salt, with ten drams of the forementioned oxygenated muriatic acid; digested the mixture, and distilled it, in twenty-four hours, with a gentle heat. Upon the fluid swam a white compact substance, and some small particles of the same kind lay at the bottom, which however rose, on the application of heat, and swam about with the rest.

*Exp.* LVIII. to LXIII. I poured the whole distillation back upon the salt, and, by means of a digesting heat, again drew off a fluid, which appeared covered with a thin fat skin. I then poured the fluid back, distilled it again, and thus repeated the process three times more. No phænomenon particularly remarkable appeared, except that the thin fat skin grew more inconsiderable, and at last seemed almost to vanish.

*Exp.* LXIV. The salt separated from the fluid, by the gentle distillation in *Exp.* LXIII. emitted now, by the force of additional heat, dark red vapours, as is usual in strong nitrous acid. When the distillation was at an end, the retort was exposed to an open fire; but, during this operation, no black matter appeared; nor was any coal separated from the mass, upon dissolving it in distilled water.*

*Exp.* LXV. I now tried the effect of a mixture of four drams of strong vitriolic acid and twelve drams of the muriatic acid, repeating the usual digestion and distillation six times. I will pass over other circumstances, and only mention, that after the sixth distillation of the fluid, a stronger heat, and at length an open fire, was applied; but hardly any fluid was produced, though the fire was so violent, that the whole mass appeared to be melted down into one uniform compact substance.

*Exp.* LXVI. The vessels having cooled, the mass was of a light milky colour throughout, without the least mixture of brown or black, or any other indication of coal.* Being some time exposed to the air, it became moist, and for a long time attracted much water, which I caused to run off. At last it remained pretty dry; but the mass seemed to have diminished, by at least one-fourth part.

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* Here the nitrous acid seemed to destroy, and carry off, the inflammable matter, sooner than it could become coal; as it had before occasioned the oily and fat substance to vanish, in the beginning of this experiment.

Here I will stop, for the present, in the description of my experiments, which sufficiently tend to prove, in a general way, the decomposition of sedative salt, and to show, that one of its component parts is inflammable matter, which may be converted into coal. I obtained of true coal, (mixed with some earth, *Exp.* xxxii. and liv.) according to the above-described experiments, (*Exp.* xxii. xxvi.—xxx.) thirty grains and three quarters, in the whole; and by other experiments, often repeated, in general, one grain and a half, more or less. Every other substance liable to be changed into coal, (as gum, tartar, sugar, &c.) suffers this change by a gentle heat, and deflagrates with nitre, in the degree of heat necessary to melt the former. But sedative salt can bear a red heat for many hours, without shewing any signs of becoming coal, of burning, or of deflagration. Astonishing phænomenon! What menstruum preserves it so securely against the assault of force, in a dissolved state, and yet suffers itself to be separated from it by more gentle means? What power exists here, to protect the inflammable particles (which afterwards turn to coal) so effectually against a degree of heat which nothing else can resist? Of what nature is the salt obtained in conjunction with the coal? These are all questions which excite great interest, but which are not easily answered. How far I have been successful in resolving them, some subsequent Essays will show; which I shall have the honour to lay before the Royal Society, as soon as I shall have sufficiently repeated the experiments I have already made.

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* Perhaps here also the remark contained in the former note holds good: yet I am rather of opinion, that the vitriolic acid did not operate with sufficient strength to separate the component parts.

# VII. *A Method of finding the Latitude of a Place, by Means of two Altitudes of the Sun and the Time elapsed betwixt the Observations. By the Rev.* W. Lax, *A. M.* Lowndes's *Professor of Astronomy in the University of Cambridge.*

Read January 10, 1799.

I hope the following method of determining the latitude, by means of two altitudes of the sun and the time elapsed betwixt the observations, will be found not less convenient for nautical purposes than the rules which are commonly employed. But I would rather recommend it in those cases where rigid accuracy is required, and the astronomer is provided with no better instrument for taking the sun's altitude than a Hadley's sextant of the most improved construction. The process will be neither difficult nor tedious; and, if the observations are made with proper exactness, I conceive the latitude will generally be obtained within a few seconds of the truth. With these expectations, I have ventured to reduce the method into its present form; and submit it, with the utmost deference, to this learned Society.

In the spherical triangle, whose sides are the complements of the latitude, declination, and altitude, let $z$ represent the angle at the pole, and $t$ its tangent; Z the azimuth, and $T$ its tangent; $L$ the latitude, and $\lambda$ its cosine, radius being unity; then, if the altitude and declination remain constant, we shall have $\dot{L} = \lambda T \dot{z}$, and, consequently, $\dot{L}$ will vary as $T \dot{z}$, when the increment of $\lambda$, compared with $\lambda$ itself, is inconsiderable. Hence, if the abscisse of the curve *ABCD* (fig. 1.) be always proportional to $z$, and its ordinate to $T$, the area *GB* intercepted betwixt any two of these ordinates may represent the increment of the latitude corresponding to the increment of the time *EG*. Let *abcd* (fig. 2.) be another curve, whose abscisse *ae* is always equal to *AE* in the preceding figure, but whose ordinate *eb* is proportional to $t$, the tangent of the hour-angle; then will the area *gb* vary as *GB*, at small distances from the meridian, and, of course, may represent the increment of the latitude. Now, to prove this, we have only to shew that $T$ and $t$, when both are small, bear to each other a given ratio. Let $S$ and $\Sigma$ be the sine and cosine of the azimuth; $s$ and $\sigma$ the sine and cosine of the angle at the pole; then will $\frac{\dot{T}}{T} = \dot{Z} \cdot \frac{1 + T^2}{T}$, and $\frac{\dot{t}}{t} = \dot{z} \cdot \frac{1 + t^2}{t}$; $\dot{Z} = \frac{\dot{S}}{\Sigma}$, and $\dot{z} = \frac{\dot{s}}{\sigma}$. But, since the complements of the declination and altitude remain constant, whilst the latitude is made to vary, $\dot{S}$ will be to $\dot{s}$ as $S$ to $s$; and, therefore, we shall have $\frac{\dot{T}}{T} : \frac{\dot{t}}{t} :: \frac{S}{\Sigma} \times \frac{1 + T^2}{T} : \frac{s}{\sigma} \times \frac{1 + t^2}{t} :: 1 + T^2 : 1 + t^2$ :: the square of the secant of the azimuth : the square of the secant of the hour-angle, which may be considered as a ratio of equality, when the angles are very small. The fluxions, therefore, of the tangents are as the tangents themselves; and, consequently, they must always preserve the same ratio towards each other. Let us now suppose that

FIG. 1.

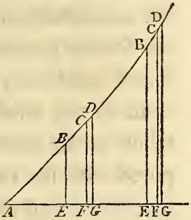
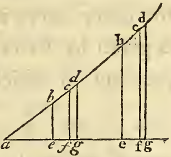


FIG. 2.



an altitude of the sun is taken at the distance *ae* from the meridian, but that, in consequence of an error in the assumed latitude, the calculated time is *ag;* and that, with a lat. differing from the former by one minute, we compute again, and the time is found equal to *af;* then will the area *gc* be to *gb* as one minute to the whole error in latitude. Let another altitude be taken at the distance *a*e from noon, and let the times computed with the two different latitudes that were employed before be *a*g and *a*f; then, in this case likewise, the area gc will be to the area gb, as one minute to the error in latitude. Now the latter curve is the "*figura tangentium,*" whose quadrature is given by COTES, in his *Harmonia Mensurarum,* and the expression for which is extremely simple. For the fluxion of the area is $= \frac{t\dot{t}}{1+t^2}$, and the area itself $=$ log. $\overline{1+t^2}|^{\frac{1}{2}} =$ log. secant of the angle at the pole. The difference of the log. secants, or log. cosines, will of course be equal to the area intercepted betwixt the tangents which correspond to them.

Hence a table might easily be constructed with a double argument,—the distance from noon, and the variation in time arising from the different suppositions of latitude,—which might immediately exhibit the logarithm of the area corresponding to any particular base *eg* supposed to be given. A second table might have for its argument the difference of the logarithms of the area *gb* and the area *gc,* (which is likewise conceived to be known) and discover at once, in degrees, minutes, and seconds, the correction to be made in the assumed latitude. This correction, as it appears from a comparison of the signs of $\dot{L}$ and $\dot{z}$ in the equation $\dot{L} = \lambda T \dot{z}$, must be added or subtracted, according as the distance from noon obtained by computation is too great or too little, when the azimuth is less than 90 degrees; but the contrary, when the azimuth exceeds a right angle. Tables of the above description shall be constructed, if this method be received with approbation; and, in the mean time, it is proposed to subjoin a short specimen which is already completed.

I have presumed that we are able to determine $eg$, the error in time arising from an error in the assumed latitude, at either of the observations; and hence it becomes necessary, before we can avail ourselves of the principles which have been laid down, to point out the manner in which this may be accomplished. The clock gives us the whole interval betwixt the observations (supposed to be made on different sides of the meridian) equal to $ae + \mathrm{ae}$, and by computation we obtain $ag + \mathrm{ag}$, and thence we deduce $eg + \mathrm{eg}$ the whole error in time. Now the area $gb$ is equal to $\mathrm{gb}$; and therefore, if we make a rough division of the whole error, without any regard to accuracy, in the inverse ratio of the hour-angles at the two observations, and, entering the first table with these times, mark the area corresponding to each at their respective distances from noon, and increase the one and diminish the other equally, till we get the areas of the same magnitude, this, we may conclude, is the proper value of each.

If the table were constructed to every second of time, we might ascertain these logarithmic areas merely from inspection; but, as it will be advisable to confine it within narrower limits, we shall sometimes find it necessary, as in other tables, to deduce their ultimate value by the rule of three. When we have increased one portion of time and diminished the other, till the difference of their corresponding areas becomes a *minimum*, we must divide this difference betwixt them, in the proportion of their respective increments in the next interval of time, and subtract or add the part assigned to each, according as it is greater or less than the other. The table, however, might easily be carried to such an extent, that exactness in this division could never be required; but, on the contrary, it would be quite sufficient, when the hour-angles were nearly equal, to add the areas together, and take half the sum for the value of each.

From these principles may be deduced the following practical rule for determining the latitude of a place. When the sun comes within fifteen degrees of the meridian, in the morning, let his altitude be taken, and the time of the observation be accurately marked; and let another altitude be taken after he has passed the meridian, whilst his distance from it is less than fifteen degrees; and let the time of this observation likewise be noted. Then, with the supposed latitude of the place, compute the times corresponding to each of the altitudes in terms of the log. cosine of the hour-angle, and take the difference of the intervals, as shewn by the clock, and determined by calculation, and divide it betwixt the observations in the manner explained above. Compute the log. cosine of the hour-angle a second time, with the greatest altitude and the latitude increased or diminished by a minute, according as it appears, from a comparison of the intervals, to have been too little or too great; and take the difference betwixt this log. cosine and that which resulted from the first operation, when the same altitude was employed. Having thus obtained the two areas $gb$ and $gc$, we must subtract their logarithms from each other, and with their difference entering the second table we shall find the degrees, minutes, and seconds, by which the assumed latitude is to be increased or diminished.

It will be needless, perhaps, to suggest that, in the higher latitudes, we may extend the limits above specified a few degrees farther from the meridian, without offering any material violence to the theory, as it has hitherto been explained; and that, on the other hand, when the declination and latitude are nearly equal and of the same denomination, it will be expedient to confine our observations within a much shorter distance from noon. But it will afterwards be demonstrated that, whatever be the magnitude of the hour-angles, or however nearly the latitude and declination may approach towards each other, we can always secure, with very little additional trouble, an exact conclusion.

We may remark that the latitude, determined in this manner, will be nearly equivalent, in point of accuracy, to the mean result of two meridian altitudes. For we know that the increment of latitude : increment of altitude : : radius : cosine of azimuth; and, since the cosine of a small angle differs so little from the radius, this may be considered, within the limits which I have prescribed, as a ratio of equality. If, therefore, one altitude of the sun were taken, and we could ascertain the error in time arising from an error in the assumed latitude without the aid of a second observation, the latitude would be discovered with nearly the same precision as if it had been deduced from the meridian altitude. But, by means of a second observation made on a different side of noon, we obtain a second error in time of the same kind; and this being added to the former, and their sum divided in a just proportion betwixt the two observations, the same effect will be produced, with respect to the accuracy of the result, as if two latitudes had been deduced from meridian altitudes, and a mean betwixt them had been taken.

I might perhaps be allowed to say more; for I am satisfied, from experience, that I can take an altitude of the sun with greater exactness, when he is in any other situation, than when he is upon the meridian. If we could ascertain, within a few seconds, or even within a minute, the time when he attains his greatest altitude, there would then be no reason why an observation should not be made with the same degree of certainty in this, as in other cases; but we are generally obliged to keep our eye stedfastly fixed, for several minutes, upon the two images, and it is well known that, in such circumstances, the best eyes are apt to be deceived. Besides, it is impossible to preserve the contact of the limbs by perpetually moving the index, whilst the sun continues to ascend so very slowly. We are compelled to wait till they are evidently separated, and then, by one turn of the screw, to bring them into contact again, which must necessarily be a source of some inaccuracy. It is for the first of these reasons that, in taking an altitude of the sun, when he is near the meridian, I have found it advisable, not, in the usual manner, to bring the images almost to touch each other, and then to wait till they actually do so, but to bring them at once into contact, with such a degree of velocity as would make them sensibly overlap, or separate, whilst the clock beats a second.

But I consider it as one of the principal advantages of this method, that we can avail ourselves of any number of altitudes, and, of course, approximate as near as we please to a true conclusion with so little additional labour. If there be an equal number of observations made on each side of the meridian, we must combine them together by pairs, according to the preceding instructions, and thus determine the different logarithmic values of *gb*. Having then added them all together, and taken a mean betwixt them, we have only to compute a single incremental area *gc* with any of the altitudes and the lat. varied one minute, and, subtracting its log. from the mean log. value of *gb*, we shall obtain a very accurate correction of the assumed latitude. But, if there be more observations on one side of the meridian than on the other, when all the pairs have been united, and the areas resulting from them found, we may combine the supernumerary observations on either side with any of those which are made on the opposite side. The properest, however, for this purpose, is the observation which is made at the least distance from the meridian.

I should hope, moreover, the practical astronomer will think it a circumstance of some moment, that the principal part of the work consists in finding the time, an operation which he is obliged so frequently to perform. Any of the three methods which are usually adopted upon this occasion might easily be applied to the tables which have been described; but I will venture to recommend a different rule, which I conceive to be better adapted to our purpose than any of the others, and to which the directions before given had a particular reference.

Let $a$ be the sine of the altitude; $\gamma$ the cosine of the hour-angle; $d$ the sine, $\delta$ the cosine, and $r$ the tangent of declination; $l$ the sine, $\lambda$ the cosine, and $s$ the tangent of the latitude. Then $\gamma = \frac{a - dl}{\delta\lambda} = \frac{a}{\delta\lambda} - \frac{dl}{\delta\lambda} = \frac{ars}{\delta\lambda rs} - rs = \overline{\frac{a}{dl} - 1} \,.\, rs$, when radius is unity, but $= \overline{\frac{am^3}{dl} - m^2} \,.\, \frac{rs}{m^3}$, when radius is $m$ $= \frac{rs}{m^3} \times^9$. into the square of the tangent of the arc whose secant is $\overline{\frac{am^3}{dl}}\Big|^{\frac{1}{2}}$. Hence we deduce the following rule for determining the log. cosine of the angle at the pole. From the log. sine of the altitude increased by three times the log. radius, subtract the sum of the log. sines of the latit. and declination; take half of the remainder, and, considering it as the log. secant of an arc, find the log. tangent corresponding; multiply this by 2, and add the log. tangents of the latit. and declin. and reject thrice the log. radius; the sum will be the log. cosine of the angle required. But, when the declin. and latit. are of a different denomination, it is evident that our expression becomes $\overline{m^2 + \frac{am^3}{dl}} \cdot \frac{rs}{m^3}$, which is equal to $\frac{rs}{m^3} \times^9$ . into the square of the secant of the arc whose tangent is $\overline{\frac{am^3}{dl}}\Big|^{\frac{1}{2}}$. In this case, therefore, having found the log. value of $\frac{am^3}{dl}$, and divided it by 2, we must consider the quotient as the log. tangent of an arc, whose log. secant being taken, we are to proceed as in the former case.

The advantages of this rule are obvious. We obtain the angle in terms of the log. cosine; and, consequently, when we have calculated the second time with the new latit. we have only to subtract one result from the other, and we immediately determine the area corresponding to the difference of the times. Besides, in the second computation, fewer of the elements are changed by this rule, than by any of those which are usually employed; and this is a consideration of much importance. But, if we are disposed to adopt the following method of ascertaining the incremental area $gc$, this advantage will be found still greater. Let us resume the expression $\gamma = \frac{a - dl}{\delta\lambda}$, and we shall have $\dot{\gamma} = \frac{1}{\delta} \times \frac{-d\lambda\dot{l} - a\dot{\lambda} + dl\dot{\lambda}}{\lambda\lambda^1}$ ($\lambda^1$ being the succeeding value of λ) $= \frac{1}{\delta} \times \frac{\frac{d\lambda^2\dot{\lambda}}{l} - a\dot{\lambda} + dl\dot{\lambda}}{\lambda\lambda^1} =$ $\frac{d\lambda^2\dot{\lambda} - al\dot{\lambda} + dl^2\dot{\lambda}}{\delta l\lambda\lambda^1} = \frac{d\lambda\dot{\lambda}}{\delta l\lambda^1} - \frac{a-dl}{\delta\lambda} \times \frac{\dot{\lambda}}{\lambda^1} = \overline{\frac{r}{s} - \gamma} \cdot \frac{\dot{\lambda}}{\lambda^1} =$ (taking $\dot{\lambda}$ positive instead of negative, as it ought to be when $\dot{l}$ is positive) $\overline{\gamma - \frac{r}{s}} \cdot \frac{\dot{\lambda}}{\lambda^1}$; and, consequently, $\frac{\dot{\gamma}}{\gamma} = \overline{1 - \frac{r}{s\gamma}} \cdot \frac{\dot{\lambda}}{\lambda^1}$, when radius is unity, but $= \overline{m^2 - \frac{rm^3}{s\gamma}} \cdot \frac{\dot{\lambda}}{\lambda^1 m^2}$, when radius is $m$. Now $\frac{\dot{\gamma}}{\gamma}$ may be considered as the increment of the hyperbolic log. of $\gamma$, and therefore, with its proper modulus, may represent the area which is the object of our investigation. We may suppose the other side of the equation to be the square of the cosine of the arc whose sine is $\left.\overline{\frac{rm^3}{s\gamma}}\right|^{\frac{1}{2}} \times^{9}$. into the increment of the hyperbolic log. of $\lambda^1$, divided by the square of the radius; and if, instead of taking this log. with the hyperbolic, we take it with Briggs's modulus, we must then consider $\frac{\dot{\gamma}}{\gamma}$ as the increment of the log. of $\gamma$, according to the same system. But $\frac{\dot{\lambda}}{\lambda^1}$ being equal to $\frac{s\dot{L}}{m^2}$ (when $\dot{L}$ is only one minute), it will vary as $s$; and, therefore, if its value be determined according to Briggs's system, when $s$ is equal to radius, and be denominated $v$, its value in any other case will be expressed by $\frac{vs}{m}$.

Hence, to obtain the log. of the area $gc$, the quantity with which we are immediately concerned, we must find the log. value of $\frac{rm^3}{s\gamma}$, and divide it by 2; we must then take out the log. cosine of the arc whose log. sine is equal to the quotient; and, having multiplied it by 2, we must add the product to the constant log. of $v$ (3,1015), and the log. tangent of the supposed latitude, rejecting thrice the log. radius. But, if $\overline{\frac{r m^3}{s \gamma}}\Big|^{\frac{1}{2}}$ be greater than radius, which must necessarily be the case when the azimuth is greater than a right angle, we must then consider $\frac{r m^3}{s \gamma} - m^2$ as the square of the tangent of the arc whose secant is $\overline{\frac{r m^3}{s \gamma}}\Big|^{\frac{1}{2}}$, observing in other respects the directions before given. The quantities $r$ and $s$ are both employed in the first computation, from the result of which we also obtain $\gamma$; and, consequently, this operation will not be attended with much trouble.

The above instructions, it is manifest, are given upon the supposition of $r$ and $s$ having the same sign; but, if the declin. and latit. should not be of a similar denomination, then will our expression become $\overline{m^2 + \frac{r m^3}{s \gamma}} \cdot \frac{v s}{m^3}$, and we must consider $m^2 + \frac{r m^3}{s \gamma}$ as the square of the secant whose corresponding tangent is $\overline{\frac{r m^3}{s \gamma}}\Big|^{\frac{1}{2}}$. With this exception, the process will be the same as when the tangents $r$ and $s$ are both affirmative.

The preceding *formula* naturally suggests to us another method of finding the log. area *gc;* and, as some perhaps may think this more eligible than either of the former, I shall take the liberty of explaining it. When the latit. is given, the area *GC*, it is obvious, must invariably preserve the same magnitude at all distances from the meridian; and, consequently, the area *gc*, which is proportional to it, must likewise remain constant. If, therefore, we can ascertain this area when the hour-angle is supposed to vanish, we may employ it when the sun is at any distance from noon. Let us now conceive the declin. to be equal to nothing; then will our expression for the area $gc$ become $\frac{\dot{\lambda}}{\lambda'}$; and, consequently, (since the tangents of the azimuth and hour-angle vanish in the ratio of their sines, or of the sines of the opposite sides in the triangle alluded to before,) we shall have the area $GC = \frac{\dot{\lambda} m}{\lambda' l}$, when the sun is upon the meridian. But this area is always the same when the latitude is given, whatever be the sun's declination, and therefore may always be represented by $\frac{\dot{\lambda} m}{\lambda' l} = \frac{v \dot{s}}{m} \times \frac{m}{l} = \frac{v m}{\lambda}$; and the area $gc$ will be generally expressed by $\frac{v m}{\lambda} \times \frac{\text{cos. of merid. altit.}}{\text{cos. of declin.}}$, when the hour-angle does not exceed the limits which have been recommended. Hence, if we add together the constant log. 3,1015, the log. radius, and the log. cosine of the merid. altitude, and subtract from their sum the log. cosines of the latit. and declin. we shall obtain the log. value of $gc$.

It will be necessary, perhaps, to meet an objection which some may be inclined to urge against the method of deducing the hour-angle in terms of the cosine, when this angle is very small. But it should be recollected, that with the angle itself we have no immediate concern, the accuracy of our conclusion depending entirely upon the accuracy with which the area corresponding to any particular increment of time can be determined. Now this area, whatever be the sun's distance from the meridian, will be nearly proportional to the increment of the latit. and, consequently, its magnitude is totally unconnected with that of the hour-angle. A given error in the quantity which expresses this area will equally affect our conclusion, whether the angle be 2, or whether it be 20 degrees. But let us inquire what effect will actually be produced, by admitting an error of half an unit in each of the log. cosines whose difference is equal to the area *gc;* and, of course, in some instances, an error of an unit in the area itself, upon any particular supposition of latit. and declin. We have only to ascertain the ratio which this area bears to unity; for the same ratio will the correction of the latit. bear to the error in our result. If the latit. for instance, be 50°, and the declin. 10°, on the same side of the equator with the latit. then, radius being unity, $\dot{z}$ (the increment of the hour-angle) will equal $\frac{\dot{L}}{\lambda T} = \frac{\dot{L}}{\cos. 50^\circ \times \frac{S}{\Sigma}} =$ ($g$ being the sine of the hour-angle, and $\alpha$ the cos. of the altit.) $\frac{\dot{L}}{\cos. 50^\circ} \times \frac{\sqrt{\alpha^2 - g^2 \delta^2}}{g \delta} = 11$ minutes nearly, when $z$ is 5°; and we have seen that, at any other distance from the meridian, the incremental area will be of the same magnitude. Hence, subtracting the log. cosine of 5° from that of 5° 11′ 0″, we get the difference equal to 1238; and, consequently, the error in our approximation will be to the error in the assumed latit. as 1 : 1238, when the log. cosines are carried to seven places of decimals.

But, when the zenith distance of the sun, at his greatest altit. is very small, and there is moreover a considerable uncertainty with respect to the latit. this error will probably become of more importance, and we may find it necessary to guard against it. Now it is manifest that, by diminishing the multiple which the area *gb* is of the area *gc*, exactly in the same proportion we shall diminish this error; and I shall afterwards explain in what manner it may be accomplished. The same expedient is also calculated to prevent another species of inaccuracy, to which, under similar circumstances, our process is liable.

If we are desirous of knowing how much our conclusion is affected by substituting the tangent of the hour-angle for the tangent of the azimuth, it may be done with the greatest facility. We have seen, that the area *GC* (fig. 1.) is to *GB* as one minute to the error in the assumed latitude. But our method supposes that *gc* (fig. 2.) is to *gb* as one minute to the correction required; and we must therefore estimate the difference betwixt these ratios, in order to ascertain how far the approximation is inaccurate. Let $EG = m \times FG$; and then, since *GC* may be considered as $= \frac{GD + FC}{2} \times FG$, and $GB = \frac{GD + EB}{2} \times EG = \frac{GD + EB}{2} \times m \times FG$, the former ratio will equal $\frac{GD + EB}{GD + FC} \times m =$ (according to the notation employed above) $m \,.\, \frac{2\mathrm{T} - m\dot{\mathrm{T}}}{2\mathrm{T} - \dot{\mathrm{T}}} = m - m \,.\, \frac{m - 1}{2} \times \frac{\dot{\mathrm{T}}}{\mathrm{T}}$ nearly $=$ the minutes contained in the computed error of latitude. Their difference will equal $m \,.\, \frac{m-1}{2} \,.\, \overline{\frac{\dot{\mathrm{T}}}{\mathrm{T}} - \frac{\dot{t}}{t}} = m \,.\, \frac{m-1}{2} \,.\, \dot{z} \,.\, \overline{\frac{1 + \mathrm{T}^2}{t} - \dot{z} \,.\, \frac{1 + t^2}{t}}$ $= m \,.\, \frac{m-1}{2} \,.\, \frac{\mathrm{T}^2 - t^2}{t} \,.\, \dot{z} = m \,.\, \frac{m-1}{2} \,.\, \frac{\mathrm{T}^2 - t^2}{\lambda \mathrm{T} t} \,.\, \dot{\mathrm{L}}$ $\Big(\frac{\dot{\mathrm{L}}}{\lambda \mathrm{T}}$ being substituted for its equal $\dot{z}\Big) =$ the minutes contained in the error of our approximation. It will be more convenient, however, to express this difference independently of the azimuth. Now, preserving the notation before adopted, we have $\mathrm{T} = \frac{\mathrm{S}}{\Sigma} = \frac{g\delta}{\alpha \times \frac{l\alpha - d}{\lambda\alpha}} = \frac{t\gamma\delta\lambda}{l\alpha - d} = \frac{t\gamma\delta}{l\gamma\delta - d\lambda}$; therefore, $\frac{\mathrm{T}}{t} = \frac{\gamma\delta}{l\gamma\delta - d\lambda}$, and $\frac{\mathrm{T}}{t} - \frac{t}{\mathrm{T}} \left(= \frac{\mathrm{T}^2 - t^2}{\mathrm{T}t}\right) = \frac{\gamma\delta}{l\gamma\delta - d\lambda} - \frac{l\gamma\delta - d\lambda}{\gamma\delta}$

$= \frac{\gamma^2\delta^2 - l^2\gamma^2\delta^2 + 2dl\gamma\delta\lambda - d^2\lambda^2}{\gamma\delta \times \overline{l\gamma\delta - d\lambda}} = \frac{\gamma^2\delta^2\lambda^2 + 2dl\gamma\delta\lambda - d^2\lambda^2}{\gamma\delta \times \overline{l\gamma\delta - d\lambda}.} =$ ($d^2\lambda^2$ being rejected, as incomparably less than the sum of the other terms,) $\lambda . \frac{\gamma\delta\lambda + 2dl}{l\gamma\delta - d\lambda} = \lambda . \frac{\gamma + 2rs}{s\gamma - r}$; and, consequently, we have $m . \frac{m-1}{2} . \frac{T^2 - t^2}{\lambda T t} . \dot{L} = m . \frac{m-1}{2} . \frac{29}{100000} . \frac{\gamma + 2rs}{s\gamma - r}$ minutes. Hence it appears that, in the latitude of Cambridge, when the sun's declination is 2° north, and his distance from the meridian 5°, this error will be equal to $m . \frac{m-1}{2} . \frac{1}{4312}$ minutes of a degree. Let the assumed differ from the true latitude ten minutes, *i. e.* let $m = 10$, then will the error amount to half a second; and, if we suppose $m = 30$, the error will not exceed four seconds.

If, instead of varying the assumed latitude one minute, we vary it $n$ minutes, in calculating the area $gc$, the above expression will be transformed into $m . \frac{m-1}{2} . \frac{29 . n}{100000} . \frac{\gamma + 2rs}{s\gamma - r}$, the increment of the latitude being, in this case, $n\dot{L}$. The error will consequently vary as $m . \frac{m-1}{2} . n$, when the declination, latitude, and time of observation are given; and, if we suppose the real to differ from the assumed latitude $p$ minutes, this last expression will become $\frac{p}{n} . \frac{\frac{p}{n} - 1}{2} . n = p . \frac{p-n}{2n}$, which varies as $\frac{p-n}{n}$, when $p$ remains constant; and, of course, the error may be diminished in any proportion by diminishing this quantity $\frac{p-n}{n}$. Now, the increment of the latitude is always $= \lambda T \dot{z}$; and, therefore, since $\dot{z}$ is determined by the process explained above, we have only to ascertain the value of $T$, in order to approximate nearly to $p$. But the sine of the azimuth is $= \frac{g\delta}{\alpha}$, which is known, and consequently the tangent may likewise be obtained. Having thus discovered a near value of $p$, or of the error in our first assumed latitude, we are enabled so far to correct it in our second hypothesis, and in a much greater degree to reduce the error in our conclusion.

As it will sometimes, though not often, be necessary to have recourse to this expedient, two short tables might be added, in order to facilitate the operation. The first might contain the log. cosines to every degree from the 15th to the 90th of the quadrant; and be so contrived, as likewise to exhibit the log. sine of any arc, as far as the 75th degree, expressed in minutes and seconds of time; that, by subtracting the log. cosine of the altitude from the log. sine of the hour-angle, (the cosine of declination being considered as equal to radius,) we might obtain the log. sine of the azimuth. This should be made one of the arguments of the second table; the other being the cosine of the latitude to every five degrees of the first sixty of the quadrant; and the table should give us the value of $15 \times \lambda T$, which, multiplied into the minutes contained in the error of time, would determine with sufficient exactness the quantity $p$. We might, indeed, with the same facility, compute the fourth table according to the mean value of the cosine of declination, if it could be supposed that such a degree of precision would ever be required.

Perhaps it would be advisable for the mariner, in order to avoid all distinction of cases, to calculate, in every instance, his incremental area with the lat. varied ten minutes, instead of one; and then he would always be secure of a result sufficiently correct for his purpose. In the case supposed above, if the real were to differ from the assumed latitude 30 minutes, the error in his conclusion would not exceed the fourth part of a second.

We must observe, that if $n$ (the approximate value of $p$, thus deduced) be the minutes by which the first assumed lat. is varied in our second hypothesis, we ought to multiply the correction, after it is taken from the 2d table, by this quantity; but, as $n$ is always supposed to be a whole number, the additional trouble arising from this process can never be an object of the smallest consideration. The 2d table, however, may be as conveniently applied to the case, where the lat. is varied ten minutes, as where it is varied only one. The logarithms which form the argument are the same in both cases, except that the index in the former is less by unity than in the latter.

Another species of inaccuracy originates in our conceiving the fluxion of the lat. to vary as $T\dot{z}$, instead of $\lambda\, T\dot{z}$; and it may be useful to ascertain how much our conclusion is affected by this circumstance. We suppose, in fact, that the increments of the lat. corresponding to the finite times $gf$ and $ge$ are to each other as the areas $gc$ and $gb$ drawn into the cosine of the first lat.; whereas, in strict propriety, they are to each other as the sum of all the elements of these areas drawn into the cosines of the latitudes belonging to each, *i. e.* (if $ge = m \times gf$) we consider these increments as being in the ratio of $gc \times \lambda : gb \times \lambda$, instead of $gc \times \frac{2\lambda + \dot{\lambda}}{2} : gb \times \frac{2\lambda + m\dot{\lambda}}{2}$. Hence

$$\frac{gb \cdot \frac{2\lambda + m\dot{\lambda}}{2}}{gc \cdot \frac{2\lambda + \dot{\lambda}}{2}} - \frac{gb}{gc} = m \cdot \frac{m-1}{2} \; \frac{\dot{\lambda}}{\lambda^{1}} = m \cdot \frac{m-1}{2} \cdot s\, \dot{\mathrm{L}} \text{ (rad.} = 1)$$

$= m \cdot \frac{m-1}{2} \cdot \frac{2gs}{100000} =$ the minutes contained in the error proceeding from this cause. When the real latitude, for instance, is 52°, and the assumed 51° 50′, this error will scarcely amount to a single second. But, if either the cosine of the latitude should be so small, or the difference betwixt the supposed and true latitude so great, as to render this error of any importance, we may prevent it by the same means that were recommended in the preceding case. In computing the incremental area $gc$, we must correct our first hypothesis respecting the latitude with the assistance of the third and fourth tables; and, for the reasons assigned above, the cause of this inaccuracy will be removed.

There is still another part of our process, which will sometimes be the source of a small error. We are directed to distribute the whole increment of the time betwixt the two observations, by equalizing the areas corresponding to each in the "*figura tangentium;*" whereas they can only be considered as perfectly equal in the original curve whose ordinates are the tangents of the azimuth. It is supposed, in reality, that $gc$ bears to $GC$ the same ratio which $\mathrm{gc}$ bears to $\mathrm{GC}$; or (which is nearly the same thing) that $gd$ is to $GD$ as $\mathrm{gd}$ to $\mathrm{GD}$: we must therefore estimate the difference betwixt these ratios, and we shall easily deduce the amount of this error. Let us retain the notation employed above, and moreover let $\Gamma$ represent the cosine, and $\tau$ the tangent of the hour-angle at the observation farthest from noon; $\dot{A}$ the difference betwixt the hour-angles, and $\dot{y}$ the excess of $\gamma$ above $\Gamma$: then, from what has already been demonstrated, we have $\frac{gd}{GD} = \frac{l\gamma\delta - d\lambda}{\gamma\delta}$, and $\frac{\mathrm{gd}}{\mathrm{GD}} = \frac{l\Gamma\delta - d\lambda}{\Gamma\delta}$;

consequently, $\frac{gd}{GD} - \frac{\mathrm{gd}}{\mathrm{GD}} = \frac{d\lambda\dot{y}}{\gamma\Gamma\delta}$ ($\dot{d}$ being neglected) will express the part of *BG*, or BG, (when they are supposed to be very small,) by which *bg* exceeds bg. Let this be multiplied into $\frac{GD}{gd}$, and it will equal $\frac{dl\dot{y}}{\Gamma . \overline{l\gamma\delta - d\lambda}} = \frac{r\dot{y}}{\Gamma . \overline{s\gamma - r}} =$ the part of *bg* itself by which it is greater than bg. But our method of equalizing the areas will take from *bg* a portion of this excess bearing to the whole the ratio of $t : t + \tau$; and, consequently, *bg* will be made too small by $\frac{r\dot{y}}{\Gamma . \overline{s\gamma - r}} \times \frac{t}{t + \tau} = \frac{rt\dot{A}}{2 . \overline{s\gamma - r}}$ nearly. Hence $\frac{mrt\dot{A}}{2 . \overline{s\gamma - r}}$ will express the minutes contained in the error arising from this cause.

The same conclusion may be deduced from the formula $\frac{\dot{\gamma}}{\gamma} = \overline{1 - \frac{r}{s\gamma}} . \frac{\dot{\lambda}}{\lambda^{1}} =$ the area *gb*, if the increment of the time be equal to *ge*. For the same reasons, $\frac{\dot{\Gamma}}{\Gamma} = \overline{1 - \frac{r}{s\Gamma}} . \frac{\dot{\lambda}}{\lambda^{1}}$ = the area gb, if the increment of the time be represented by ge. Hence $gb - \mathrm{gb} = \frac{\dot{\gamma}}{\gamma} - \frac{\dot{\Gamma}}{\Gamma} = \frac{r\dot{y}'}{s\gamma\Gamma} \times \frac{\dot{\lambda}}{\lambda^{1}}$, and $\frac{gb - \mathrm{gb}}{gb} \times \frac{t}{t + \tau} = \frac{r\dot{y}}{\Gamma . \overline{s\gamma - r}} \times \frac{t}{t + \tau} =$ that part of the whole area *gb* by which it is made to exceed its proper magnitude; and $\frac{mrt\dot{A}}{\overline{2s\gamma - r}} =$ the minutes contained in the error which we are investigating. If, for instance, the latitude be 52° 12′, and the declination 2°, both of the same kind, and one of the altitudes be taken at the distance of 5°, the other at the distance of 10°, from the meridian, this error will amount to $\frac{1}{10000}$th part of the whole difference betwixt the real and the assumed latitude. Hence, if this difference be ten minutes, the error in the approximation will not be more than $\frac{1}{17}$th part of a second; and, if the difference be one degree, the error will only be six times greater than in the former case. It will therefore always be inconsiderable, except when the latitude and declination are nearly equal, and on the same side of the equator.

But, if this cause should be likely to produce an effect of some importance, we may prevent it by computing an incremental area with the least altitude likewise, and the latitude increased or diminished by a minute, and then taking a mean betwixt them both for the magnitude of the area *gc*. Thus we shall generally obtain a conclusion sufficiently exact; but, if we are desirous of rendering it perfectly so in every instance, we must divide the difference betwixt the real and apparent intervals, in such a manner that the areas assigned to the two observations may be to each other in the same ratio as their correspondent incremental areas. This division will be readily dispatched by making the difference betwixt the logarithms of the two first terms, in the proportion (taken from the 1st table) equal to the difference betwixt the logarithms of the two last, which are given when either of the formulæ $\overline{m^2 - \frac{r\,m^3}{s\,\gamma}} \cdot \frac{v\,s}{m^3}$, or $\frac{v\,m}{\lambda} \times \frac{\text{cos. of merid. alt. } (\mu)}{\text{cos. of declin. } (\delta)}$, is employed; but must be found, if the first method of computing the area *gc* be adopted. Suppose that *a* and *b*, representing the logs. of the areas *gb* and gb in the table, appear from inspection to exceed each other nearly as much as the logs. of the areas *gc* and gc; let *c* and *d* be the two next terms in succession, and let *m* and *n* be the logs. of the quantities expressing the incremental areas; then will

$\overline{a \sim b} \sim \overline{c \sim d} : \overline{a \sim b} \sim \overline{m \sim n} :: \overline{a \sim c}$ : the quantity to be added to, or subtracted from, $a$, to make it of the required magnitude. We have then only to subtract $m$ from this corrected value of $a$, to proceed with the remainder to the second table, and take out the corresponding error in the assumed latitude. This precaution, however, can very seldom be necessary; and, even when it is deemed advisable to adopt it, the division may be performed with so little regard to exactness as to render the process easy and expeditious.

But we are not to conclude that, because this inaccuracy, and also the two first species that were considered, are likely to be introduced, when the latitude and declination are nearly equal and of the same kind, they will therefore unavoidably exist in these circumstances. On the contrary, they may always be prevented, when the weather is favourable, by making the observations within a smaller distance from the meridian. We have only to wait till the increase of the sun's altitude becomes so slow as not to produce a visible separation of the limbs in two or three seconds, and then we may be assured that the azimuth is small, and consequently that none of these errors will be considerable.

I have hitherto supposed that this method is only to be adopted, when the sun, at each observation, is within fifteen degrees of the meridian; or (to speak more accurately) when both the azimuth and the hour-angle are so small that we may consider their tangents as bearing a given ratio to each other; and, indisputably, these limits should never be transgressed, when it can possibly be avoided; for we have seen (page 79th) that, whatever be the method employed, the smaller the hour-angle, the greater is the exactness with which the lat. is determined. Sometimes, however, it will be impossible to make both, or perhaps either of our observations within the distance which I have recommended; but, even in these cases, our rule may be conveniently applied. It has already been demonstrated that we can never be subject to any material error in consequence of the inequality of the areas *gb* and gb, except when the zenith-distance of the sun, at his meridian altitude, is very small; and, for this case, an effectual remedy has been provided. We need not, therefore, make any farther remarks upon this species of inaccuracy.

But perhaps it will be imagined, that because we still continue to suppose the areas of the "*figura tangentium*" to represent the increments of the latitude, a considerable error will be introduced. We can easily prove, however, that in consequence of the increment of the time being so much diminished by increasing the distance from noon, this error will seldom be of moment enough to claim our attention. Let $\mathrm{T}$ be the tangent of the azimuth, and $\tau$ the tangent of the hour-angle at the observation farthest from noon; then it follows, from what has been demonstrated before, that the ratio of gb to gc will exceed the ratio of GB to GC, and, consequently, the ratio of *gb* to *gc*, (supposed to be of a proper magnitude,) by $m \,.\, \frac{m-1}{2} \,.\, \frac{\mathrm{T}+2rs}{s\mathrm{T}-r}\,\dot{\mathrm{L}}$. Hence it is evident that, by diminishing one of these ratios and increasing the other, as we are directed in the present case, till they become equal, we augment the latter by a portion of this difference expressed by $\frac{t}{t+\tau}$, and, on this account, it will be made too great by $m \,.\, \frac{m-1}{2} \,.\, \frac{t}{t+\tau} \,.\, \frac{\mathrm{T}+2rs}{s\mathrm{T}-r}.\,\dot{\mathrm{L}}$.

But the ratio of $gb$ to $gc$ will itself exceed the ratio of $GB$ to $GC$, its proper value, by $m \,.\, \frac{m-1}{2} \,.\, \frac{\gamma + 2rs}{s\gamma - r} \,.\, \dot{L}$; and, by the method of equalizing the ratios, it is suffered to retain a portion of this difference expressed by $\frac{\tau}{t+\tau}$; and, therefore, it will be made too great by $m \,.\, \frac{m-1}{2} \,.\, \frac{\tau}{t+r} \,.\, \frac{\gamma + 2rs}{s\gamma - r} \,.\, \dot{L}$. The sum of these quantities, $m \,.\, \frac{m-1}{2} \,.\, \frac{29}{100000} \,.\, \frac{1}{t+\tau} \,.\, \overline{\frac{t\Gamma + 2rst}{s\Gamma - r} + \frac{\tau\gamma + 2rs\tau}{s\gamma - r}}$, will determine, in minutes of a degree, the amount of the whole error, when the incremental area $gc$ represents one minute of latitude. But if, in our second hypothesis, the latitude be varied $n$ minutes, instead of one, and $p$ be the correction required, the above expression becomes $p \,.\, \frac{p-n}{2n} \,.\, \frac{29}{100000} \,.\, \frac{1}{t+\tau} \,.\, \overline{\frac{t\Gamma + 2rst}{s\Gamma - r} + \frac{\tau\gamma + 2rs\tau}{s\gamma - r}}$ minutes. Hence, in the lat. of Cambridge, when the declination is 2° north, and the sun's distance from the meridian at one observation is 5°, and at the other 45°, $p$ being equal to 10, and $n$ equal to unity, the error will amount to little more than half a second.

But, whenever it is judged necessary to guard against this species of error, we need only diminish the value of $\frac{p-n}{n}$ in the last expression; and this will readily be effected by dividing the difference betwixt the real and computed intervals of time, in such a manner that the portions belonging to the two observations may be to each other inversely as the quantities taken out of the fourth table, according to the instructions before delivered. When the portion belonging to the greatest altitude is thus obtained, we can immediately deduce the correction to be made in the first assumed latitude, and then proceed to calculate the incremental areas. Let us suppose, for instance, the difference betwixt the intervals to be *q*, and the quantities deduced from the 4th table to be *a* and *b*; then will $\frac{abq}{a+b}$ = the minutes by which the former latitude is to be varied.

If, however, there be any considerable uncertainty respecting the latitude, I would recommend the following method of obtaining an approximate value of it, before we begin the process for investigating its real magnitude. This will effectually preclude, in almost every instance, the various errors that have been described. The increment of the altitude is equal to $\lambda Sz$, and, therefore, when the azimuth is so small that its sine varies nearly as the arc, the whole increase of the altitude, whilst the sun is moving to the meridian, will be $\lambda z \times \frac{S}{2} = \frac{g^2\lambda\delta}{2\alpha}$, the sine of the hour-angle being considered as equal to the arc itself. Now the first of the two tables which have just been explained, will enable us to find the value of $\frac{g^2}{\alpha}$, by subtracting the log. cosine of the altitude from twice the log. sine of the hour-angle; and a third table might be added, to furnish us with the whole expression $\frac{g^2\delta\lambda}{2\alpha}$, (according to the mean value of $\delta$,) one of its arguments being the quantity $\frac{g^2}{\alpha}$ already determined, and the other the supposed latitude. It might, perhaps, be advisable to add another column to the first of these tables, containing twice the logarithmic sine of the hour-angle, as this would in some measure abridge the operation. We should find it more convenient too, if the last table were to give us the complement of the arc whose value is $\frac{g^2\delta\lambda}{2a}$, rather than the arc itself; because it would then only be necessary to subtract the observed altitude from this complement, and we should immediately deduce the zenith distance of the sun, when he had arrived at the meridian. This being ascertained, we should have no farther difficulty in finding the latitude to be adopted in the subsequent computation. The exactness of the conclusion which is derived from this process, will necessarily depend upon the degree of certainty with which the time is given when the observation is made. An error in time may arise, both from an irregularity in the going of the clock and a small inaccuracy in estimating the difference betwixt the longitude of the place where the altitude is now taken, and that where the time was last determined; but these causes, it is evident, can seldom be very considerable. We may generally, I think, be sure of the time within two or three minutes.

The principle upon which this approximation depends will open to us a more compendious way of finding the area *g b*, and which may always be pursued with advantage, when one of the altitudes can be taken at a small distance from the meridian. In the higher latitudes, indeed, the hour-angle may amount to five or six degrees; but, when the latitude and declination approach towards an equality, and are of the same denomination, this angle must be restrained within narrower limits. If, instead of deducing the meridian altitude from the altitude observed, according to the directions before given, we consider it as being actually equal to the meridian altitude, and employ the latitude resulting from this hypothesis in computing the hour-angle, this angle must necessarily be found equal to nothing. We may, therefore, spare ourselves the trouble of performing this part of the operation, and need only calculate the time belonging to the second observation, which is supposed to be made when the sun is at a greater distance from the meridian. The error in the assumed latitude will, in this case, be equal to the whole increase of the altitude betwixt the time of observation and noon; and from this consideration we may be enabled, in any particular instance, to discover at what distance from the meridian this plan may be safely adopted. We can extend it also to any number of observations that happen to be made within the proper limits, by connecting them successively with one on the opposite side of the meridian, and thus determining the areas $gb$ corresponding to each. Having afterwards calculated an incremental area with any of the assumed latitudes varied one minute, we must subtract its logarithm from the logarithmic value of each of the areas $gb$, and we shall discover, by means of the second table, the correction to be applied to each of the supposed latitudes.

But the method which has just been explained might always be safely adopted, within the limits prescribed to the former rule, if, in computing the incremental area, we were to employ the latitude obtained by means either of the 3d and 4th, or of the 3d and 5th tables. This precaution would entirely prevent the error which might arise from the inequality of the ratios of $GC$ to $GB$, and $gc$ to $gb$, when the difference betwixt the real and the assumed latitude was very considerable. The error which is occasioned by the disparity of the hour-angles, might be obviated by the same means that were practised in the first method. There is only one objection, indeed, to this rule's being exclusively adopted, when either of the altitudes is taken within the distance at first recommended. It would be necessary, in that case, to extend the table much farther than we are obliged to do, if we adhere to the former rule; but the labour is so much abridged by this plan, that I am doubtful whether its advantages might not be more than sufficient to compensate this inconvenience. The table should at least be carried to such a length as would enable us to proceed in this manner, whenever an observation was made within five or six degrees of the meridian.

I must not forget to observe, before I conclude the theory, that although I have directed the altitudes to be taken on different sides of the meridian, it is by no means requisite that we should invariably adhere to this precept. We have seen the reason, indeed, why it is expedient, in most instances, to prefer this method, as being generally calculated to produce a much greater degree of exactness in the result. This, however, is not always the case; for, if one of the observations be made beyond the distance originally prescribed, it is of little importance whether the second altitude be taken on the same side of the meridian, or not. But it will sometimes be impossible to make the observations on different sides of noon; and hence it becomes necessary to inquire in what manner the real latitude may be discovered in these circumstances. The clock gives us the interval betwixt the observations equal to $\overline{\mathrm{ae} - ae}$; and by computation we find $ag$ and $\mathrm{ag}$, and thence we deduce $\overline{eg - \mathrm{eg}}$, the difference betwixt the errors in time. Having then assumed, without any regard to accuracy, two portions of time, corresponding to the two observations, whose difference is the same as the difference betwixt the errors before determined, and which are to each other in the inverse ratio of the hour-angles, we must increase or diminish them both equally, till we get the areas in the first table of the same magnitude, and then we may conclude that we have obtained the proper value of each. The directions which have been given for the prevention of errors in the former case, when the altitudes are taken on different sides of the meridian, are very easily accommodated to the present; and it would therefore be superfluous to bestow any farther consideration upon them.

From a review of the inaccuracies to which this method, in particular cases, may be liable, it appears that none of them can ever be of sufficient importance to affect the mariner. If he only computes the time with each of the altitudes and the latitude by account, and an incremental area with the greatest altitude and the former latitude varied ten minutes, the correction will generally be deduced within much less than a second, and, in the most unfavourable circumstances, within a minute, of the truth. But the astronomer, in every instance, even when the latitude and declination are nearly equal and of the same kind, by adopting the precautions which have been recommended, may be assured of a result perfectly exact. If, however, he should entertain any doubts upon this point, he might easily compute a second value of the incremental area with the latitude already determined; and this, it is evident, would necessarily produce a conclusion not less accurate than if it were obtained from the direct method.

The most satisfactory way of proving the utility of this rule, will be to suppose a particular latitude and declination; with these to compute the altitudes, when the sun is at two given distances from the meridian; and thence to deduce the latitude, by an application of our own principles.

### EXAMPLE I.

Let the latitude be 54° 27′ 50″ north, and the declin. when the first altit. is taken, 2° 22′ 32″, and, when the second is taken, 2° 21′ 35″, (of the same denomination as the latit.) one observation being made in the morning, when the sun is 5° 3′ 22″, and the other in the afternoon, when he is 10° 1′ 10″, distant from the meridian. The altitude in the former case will be found equal to 37° 44′ 52″; and in the latter to 37° 15′ 20″.

| | Observation 1st. | | Observation 2d. |
|---|---|---|---|
| | Lat. 54° 15′ 0″ | Lat. 54° 16′ 0″ | Lat. 54° 15′ 0″ |
| Log. of $a$ = | 9,7868838 | 9,7868838 | 9,7820217 |
| — $d$ = | 8,6175181 | 8,6175181 | 8,6146155 |
| — $l$ = | 9,9093281 | 9,9094190 | 9,9093281 |
| — $dl$ = | 18,5268462 | 18,5269371 | 18,5239436 |
| — $\frac{am^3}{dl}$ = | 21,2600376 | 21,2599467 | 21,2580781 |
| — $\overline{\frac{am^3}{dl}}\Big\rvert^{\frac{1}{2}}$ = | 10,6300188 | 10,6299733 | 10,6290390 |
| Log. tang. = | 10,6177463 | 10,6176983 | 10,6167094 |
| 2 × log. tang. = | 21,2354926 | 21,2353966 | 21,2334188 |
| Log. of $r$ = | 8,6178915 | 8,6178915 | 8,6149839 |
| — $s$ = | 10,1427296 | 10,1429961 | 10,1427296 |
| Computed log. of $\gamma$ = | 9,9961137 | 9,9962842 | 9,9911323 |
| True — = | 9,9983068 | | 9,9933253 |
| Area $gb$ = | 21931 | 9,9961137 | Area $gb$ = 21930 |
| | | Area $gc$ = 1705 | |

Hence, $\frac{gb}{gc} = \frac{21931}{1705} = 12' \, 51''$, and latit. = 54° 27′ 51″.

If we employ the formula $\frac{\dot{\gamma}}{\gamma} = \overline{m^2 - \frac{rm^3}{s\gamma}} \cdot \frac{vs}{m^3}$ in computing the area $gc$, we shall obtain, very nearly, the same result.

Log. of $r$ = 8,6178915
— $s$ = 10,1427296
— $\gamma$ = 9,9961139
— $s\gamma$ = 20,1388435
— $\frac{rm^3}{s\gamma}$ = 18,4790480
— $\overline{\frac{rm^3}{s\gamma}}\Big|^{\frac{1}{2}}$ = 9,2395240
Log. cosine = 9,9933560
2 × log. cos. = 19,9867120
Const. log. = 3,1015
Log. $s$ = 10,1427296
— $gc$ = 3,2309416
— $gb$ = 4,3410188
Difference = 1,1100772, producing in tab. 2d. 12′ 52″
Lat. corrected = 54° 27′ 52″

But there is no occasion to write down the logs. of $r$, $s$, and $\gamma$, again: we may add together the logs. of $s$ and $\gamma$, and subtract their sum from the log. of $rm^3$ at the same time, as they stand in the former operation, and this will render the latter process extremely short. Let us resume the preceding example, and compute the area $gb$ by the second method, and the area $gc$ by the formula $\overline{m^2 - \frac{rm^3}{s\gamma}} \cdot \frac{vs}{m^3}$.

| | | |
|---|---|---|
| | | Complement of alt. 52° 15′ 8″ |
| Observation 1st. | Observation 2d. | Declin. 2 22 32 |
| Lat. 54° 37′ 40″. | Lat. 54° 37′ 40″. | Lat. 54 37 40 |
| | Log. of $a$ = 9,7820217 | |
| | — $d$ = 8,6146155 | |
| | — $l$ = 9,9113753 | |
| | — $\frac{a\,m^3}{d\,l}$ = 21,2560309 | |
| | — $\left.\frac{a\,m^3}{d\,l}\right\|^{\frac{1}{2}}$ = 10,6280154 | |
| | Log. tang. = 10,6156259 | |
| | 2 × log. tang. = 21,2312518 | Log. of $\frac{r\,m^3}{s\,\gamma}$ = 18,4711836 |
| | Log. $r$ = 8,6149839 | $\left.\frac{r\,m^3}{s\,\gamma}\right\|^{\frac{1}{2}}$ = 9,2355918 |
| | — $s$ = 10,1487823 | Log. cosine = 9,9934771 |
| Computed log. $\gamma$ = 10,0000000 - - - | 9,9950180 | 2 × log. cos. = 19,9869542 |
| True - = 9,9983068 - - - | 9,9933253 | Const. log. = 3,1015 |
| Area $g\,b$ = 16932 | Area gb = 16927 | Log. area $g\,c$ = 3,237 |
| $g\,b$ + gb = 33859 | | |
| Mean value of $g\,b$ = 16929 | | |
| Whose log. (from tab. 1st) - = 4,229 | | |
| $g\,c$ = 3,237 | | |
| Difference = 1992 | producing from tab. 2d - - - - | 9′ 49″ |
| | | Lat. corrected = 54° 27′ 51″ |

EXAMPLE II.

Let the real latitude be 10° 0′ 0″, and the two observations be made on different sides of noon; one when the sun's distance from the meridian is 5° 0′ 0″, and the other when his distance is 10° 0′ 0″; the declin. in the former case being 7° 40′ 40″, and in the latter 7° 39′ 43″, of the same kind as the latitude. Then will the greatest altitude be 84° 32′ 28″, and the least 79° 50′ 48″.

| | Observation 1st. | | Observation 2d. |
|---|---|---|---|
| | Lat. 10° 10′ 0″ | Lat. 10° 9′ 0″ | Lat. 10° 10′ 0″ |
| Log. of $a$ = | 9,9980259 | 9,9980259 | 9,9931449 |
| —— $d$ = | 9,1258124 | 9,1258124 | 9,1249212 |
| —— $l$ = | 9,2467746 | 9,2460695 | 9,2467746 |
| —— $\frac{a\,m^3}{d\,l}$ = | 21,6254389 | 21,6261440 | 21,6214491 |
| —— $\left.\frac{a\,m^3}{d\,l}\right\|^{\frac{1}{2}}$ = | 10,8127194 | 10,8130720 | 10,8107245 |
| Log. tang. = | 10,8075134 | 10,8078745 | 10,8054699 |
| 2 × log. tang. = | 21,6150268 | 21,6157490 | 21,6109398 |
| Log. of $r$ = | 9,1297233 | 9,1297233 | 9,1288160 |
| —— $s$ = | 9,2536477 | 9,2529200 | 9,2536477 |
| Computed log. of $\gamma$ = | 9,9983978 | 9,9983923 | 9,9934035 |
| True —— = | 9,9983442 | | 9,9933515 |
| | Area $gb$ = 536 | 9,9983978 | Area gb = 520 |
| | | Area $gc$ = 55 | |

$\frac{gb + gb}{2} = 528$. Hence, correction $= \frac{528}{55} = 9' \, 34''$, and lat. corrected = 10° 0′ 26″.

The difference, however, betwixt the real and the corrected lat. would only have amounted to 19″, if we had determined the area *gb* by the 1st table, instead of taking a mean betwixt the two areas for its proper value. This error might have been expected from the near approach of the lat. to the declin. and ought therefore to have been guarded against. It proceeds from the three causes of inaccuracy which, I have shewn, must necessarily be combined in these circumstances. The remedies to be applied are well known. We must find an approximate value of $p$ (page 89) by means of the 3d and 4th tables, and compute an incremental area with the least, as well as with the greatest altitude. If this plan had been pursued, the latitude would have been ascertained within less than a second of the truth. Or, if the weather had been favourable, we might have prevented the error as effectually, by making the observations when the hour-angles were much less, and approached nearer to an equality.

*N. B.* The log. sines and tangents of the declin. and lat. to be employed in all the operations should be taken out at the same time. But, when the first method of computing the incremental area is adopted, we may avail ourselves, with considerable advantage, of the following expedient. Instead of taking the whole log. sine and tangent of the new lat. take the increments of each, or the difference betwixt their respective values in the two suppositions; and find the increment of the log. tangent corresponding to the increment of the log. secant $\overline{\frac{am^3}{dl}\Big|}^{\frac{1}{2}}$. This may readily be effected, when the log. tangent is deduced from the log. secant in the former process, by taking the rate of increase belonging to each, and thence inferring by an easy proportion the proper increment of the log. tangent in the latter case. The difference betwixt this increment and the increment of $r$ is the value of the area $gc$. An instance of the method thus improved shall be given in the next example.

## EXAMPLE III.

Let us suppose the true lat. to be 54° 27′ 50″, and two observations to be made in the afternoon—one when the sun's distance from the meridian is 4° 30′ 0″, the other when his distance is 45° 10′ 15″—the declin. in the former case being 8° 7′ 35″, and in the latter 8° 5′ 3″, on the same side of the equator with the latitude. Then will the greatest altitude be found equal to 43° 31′ 19″, and the least to 31° 20′ 25″.

| | Observation 1st. Lat 54° 10′ 0″ | | Observation 2d. Lat. 54° 10′ 0″ | | Observation 1st. Lat. 54° 11′ 0″ | | Observation 2d. Lat. 54° 11′ 0″ |
|---|---|---|---|---|---|---|---|
| Log. of $a$ = | 9,8379894 | - | 9,7161022 | | | | |
| —— $d$ = | 9,1503179 | - | 9,1480707 | | | | |
| —— $l$ = | 9,9088727 | - | 9,9088727 | | | | |
| —— $\frac{a\,m^3}{d\,l}$ = | 20,7787988 | - | 20,6591588 | Incr. of log. of $l$ = | 912 | - | 912 |
| —— $\left.\frac{a\,m^3}{d\,l}\right\|^{\frac{1}{2}}$ = | 10,3893994 | - | 10,3295794 | —— log. secant = | 456 | - | 456 |
| Log. tang. = | 10,3498734 | - | 10,2758492 | —— tang. = | 545 | - | 584 |
| 2 × log. tang. = | 20,6997468 | - | 20,5516984 | 2 × incr. of log. tang. - = | 1090 | - | 1168 |
| Log. of $r$ = | 9,1547009 | - | 9,1524081 | | | | |
| —— $s$ = | 10,1413981 | - | 10,1413981 | Incr. of log. of $s$ = | 2662 | - | 2662 |
| Computed log. of $\gamma$ = | 9,9958458 | - | 9,8455046 | Area $gc$ = | 1572 | .. gc = | 1494 |
| True —— = | 9,9986590 | - | 9,8481862 | | | | |
| | Area $gb$ = 28132 | | Area gb = 26816 | | | | |

Now, by diminishing $gb$ and gb, till the ratio of $gc$ to gc becomes equal to the ratio of $gb$ to gb, we should get $gb = 28126$, and gc = 26735. Hence $\frac{gb}{gc} = \frac{28126}{1572} =$ 17′ 53″ the correction, and the latitude = 54° 27′ 53″.

But the same conclusion would have been obtained independently of the second incremental area, which I have demonstrated above can never be necessary, except when the zenith distance at noon is very small. The error of three seconds arises from a different cause, and might have been entirely excluded with the assistance of the 3d and 4th tables. We should, in fact, deduce from these tables an approximation of 22′; and, if we calculate the area *gc* with the lat. 54° 32′ 0″, it will be found equal to 34762, and consequently $\frac{28172}{36762}$ = 48″,62, which, multiplied by 22, produces 17′ 49″,64 for the correction, and the lat. is found = 54° 27′ 49″,64.

It would have been still better, however, to have obtained a near value of the lat. in the first instance, by means of the 3d and 5th tables. The work might have been dispatched in rather less time, and the conclusion would have been as rigidly accurate.

It is obvious that, if the time when the first observation was made could have been ascertained within two or three seconds, the area *gb* might have been immediately found by either of the two methods which have been explained, without the assistance of a second altitude, or of the first table; and the latitude determined with much greater facility, and with sufficient exactness. When the azimuth indeed, as in the present case, does not exceed four or five degrees, an error of a second in time will not produce an error of more than a second in the result; and, as the azimuth decreases, the error in latitude, arising from a given error in time, will be diminished in the same proportion. In general, if $\dot{z}$ be the error in the assumed time, the error in the corrected latitude will be nearly equal to $15 T \lambda \dot{z}$.

Let us then suppose the time to be given at the first observation, and let us determine the area $gb$ by the second method, and the area $gc$ by the formula $\frac{vm}{\lambda} \cdot \frac{\text{cos. of merid. alt. } (\mu)}{\text{cos. of declin. } (\delta)}$.

Comp. of alt. = 46° 28′ 41″
Declin. = 8 7 35
Lat. assumed = 54 36 16

Computed log. of $\gamma$ = 10,0000000
True —— = 9,9986591
Area $gb$ = 13409
Log. of area $gb$ = 4,127
———— $gc$ = 3,203

Log. of $v$ = 3,1015
—— $\mu$ = 9,8604043
—— $mv\mu$ = 22,9619043
—— $\delta$ = 9,9956171
—— $\lambda$ = 9,7628420
—— Area $gc$ = 3,2034452

Difference = ,924 producing from tab. 2d. 8′ 23″½.
Lat. corrected = 54° 27′ 52″½.

### EXAMPLE IV.

The latitude, which is supposed in the last example to be 54° 27′ 50″, is that of Ravensworth, a village about five miles to the north of Richmond, in Yorkshire, where I resided some months in the summer of 1797. During that time, I neglected no opportunity of taking the sun's meridian altitude; and, accordingly, the latitude which is there assumed is the mean result of a considerable number of observations. Moreover, on the 8th of September, I took, with particular care, four altitudes, when the sun was near the meridian; and with these I will now calculate the latitude, in order to apply the 1st and 2d tables, and likewise to exemplify the method of combining several observations together. The two first altitudes were taken in the morning, one at 19′ 24″, the other at 17′ 40″ before twelve o'clock, and were respectively 40° 49′ 6″, and 40° 50′ 51″; the two last in the afternoon, one at 17′ 53″, the other at 19′ 44″ past twelve o'clock, and were respectively 40° 49′ 50″, and 40° 48′ 18″. Now the longitude, found by the eclipses of Jupiter's satellites, is nearly 1° 40′ west of Greenwich; and hence the declin. appears to have been, at the two first observations, 5° 26′ 25″, and at the two last, 5° 25′ 47″; the interval betwixt the first and second, and betwixt the third and fourth, being so small that we may consider the declin. as remaining constant during each of these times.

| | Observation 1st. | Observ. 2d. | Observ. 3d. | Observ. 4th. | | |
|---|---|---|---|---|---|---|
| | Lat. 54° 29′ | Lat. 54° 29′ | Lat. 54° 29′ | Lat. 54° 29′ | | |
| Log. of $a =$ | 9,8153538 | 9,8156096 | 9,8154610 | 9,8152367 | | |
| —— $d =$ | 8,9768457 | 8,9768457 | 8,9760047 | 8,9760047 | | |
| —— $l =$ | 9,9105959 | 9,9105959 | 9,9105959 | 9,9105959 | | |
| —— $\frac{a\,m^3}{d\,l} =$ | 20,9279122 | 20,9281680 | 20,9288604 | 20,9286361 | Log. $\frac{r\,m^3}{s\,\gamma} =$ | 18,8300552 |
| —— $\overline{\frac{a\,m^3}{d\,l}}\Big\rvert^{\frac{1}{2}} =$ | 10,4639561 | 10,4640840 | 10,4644302 | 10,4643180 | —— $\overline{\frac{r\,m^3}{s\,\gamma}}\Big\rvert^{\frac{1}{2}} =$ | 9,4150276 |
| Log. tang. $=$ | 10,4366767 | 10,4368218 | 10,4372141 | 10,4370861 | Log. cosine $=$ | 9,9847972 |
| 2 × log. tang. $=$ | 20,8733534 | 20,8736436 | 20,8744282 | 20,8741722 | 2 × $l$ cos. $=$ | 19,9695944 |
| Log. of $r =$ | 8,9788063 | 8,9788063 | 8,9779578 | 8,9779578 | Log. $v =$ | 3,1015 |
| —— $s =$ | 10,1464648 | 10,1464648 | 10,1464648 | 10,1464648 | —— $s =$ | 10,1464648 |
| Comput. log. $\gamma =$ | 9,9986245.. | 9,9989147.. | 9,9988508.. | 9,9985948. | Log. $g\,c =$ | 3,218 |

| | | | | |
|---|---|---|---|---|
| 1st observation. | Time from the argument of 1st table - | | 18′ 14″, by clock | 19′ 24″ |
| 4th, ——— | ——— | | 18 26 ——— | 19 44 |
| | Near log. value of *gb* from 1st table | 3,296 | 36 40 | 39 8 |
| | ——— gb ——— | 3,276 | | 36 40 |
| | | 2)6,572 | Error in time = | 2 28 |
| | First log. value of *gb* - - | 3,286 | | |
| 2d observation. | Time from the argument of 1st table - | | 16′ 12″, by clock | 17′ 40″ |
| 3d ——— | ——— | | 16 40 ——— | 17 53 |
| | | | 32 52 | 35 33 |
| | Near log. value of *gb* from 1st table | 3,294 | | 32 52 |
| | ——— gb ——— | 3,264 | Error in time = | 2 41 |
| | | 2)6,558 | | |
| | Second log. value of *gb* - - - | 3,279 | | |
| | Mean ——— - - | 3,282 | | |
| | Log. value of *gc* - - - | 3,218 | | |
| | Difference | ,064 | producing in 2d tab. | 1′ 10″ |
| | | | Lat. corrected = | 54° 27′ 50″ |

We now take the same example again, and apply the second method of computing the area *gb*, and the formula $\frac{m v \mu}{\delta \lambda}$ in determining the area *gc*.

| | | | |
|---|---|---|---|
| Comp. of alt. at 2d observ$^n$ | 49° 9′ 9″ | Comp. of alt. at 3d observ$^n$ | 49° 10′ 10″ |
| Declin. ———— | 5 26 25 | Declin. ———— | 5 25 47 |
| Lat. = | 54 35 34 | Lat. = | 54 35 57. |

| | Observations 1st and 3d. | | Observations 4th and 2d. |
|---|---|---|---|
| | Lat. 54° 35′ 57″ | | Lat. 54° 35′ 34″ |
| Log. of $a$ = | 9,8153538 | - - | 9,8152367 |
| —— $d$ = | 8,9768457 | - - | 8,9760047 |
| —— $l$ = | 9,9112212 | - - | 9,9111868 |
| —— $\frac{am^3}{dl}$ = | 20,9272869 | - - | 20,9280452 |
| —— $\overline{\frac{am^3}{dl}}\Big\vert^{\frac{1}{2}}$ = | 10,4636434 | - - | 10,4640226 |
| Log. tang. = | 10,4363220 | - - | 10,4367520 |
| 2 × log. tang. = | 20,8726440 | - - | 20,8735040 |
| Log. of $r$ = | 8,9788063 | - | 8,9779578 |
| —— $s$ = | 10,1483229 | - - | 10,1482204 |
| Computed log. of $\gamma$ = | 9,9997732 | - - | 9,9996822 |
| True —— = | 9,9984422 | - - | 9,9983881 |
| Area $gb$ = | 13310 | - - | 12941 |
| Computed log. of $\Gamma$ = | 10,0000000 | - - | 10,0000000 |
| True —— = | 9,9986765 | - - | 9,9987084 |
| Area gb = | 13235 | - - - | 12916 |
| $\frac{gb + \text{gb}}{2}$ = | 13272 | - - | 12928 |
| Log of $\frac{gb + \text{gb}}{2}$ = | 4,1229 | - - | 4,1115 |
| —— $gc$ = | 3,2194 | - - - | 3,2194 |
| Difference = | ,9039 | - - | ,8921 |
| Hence, correction from tab. 2d - = | 8′ 1″ | - - - | 7′ 48″ |
| Lat. corrected = | 54° 27′ 56″ | - - | 54° 27′ 44″ |

Mean corrected lat. = 54° 27′ 50″

| | |
|---|---|
| Log. $v$ = | 3,1015 |
| —— $\mu$ = | 9,8787 |
| $m v \mu$ = | 22,9803 |
| —— $\delta$ = | 9,9980 |
| —— $\lambda$ = | 9,7629 |
| —— Area $gc$ = | 3,2194 |

## REMARKS.

1. All the altitudes that are taken on the same side of noon, tend only to correct the error which may be supposed to exist in the greatest of these altitudes, and can have no effect in removing any inaccuracy to which the greatest altitude on the other side of the meridian may be subject. Hence we must take more than one altitude on each side of noon, if we are desirous of obtaining a very exact conclusion.

2. When some of the observations are made in the morning, and others in the afternoon, the smaller the hour-angle, in every instance, the more favourable it will be for our purpose. But, if we cannot procure an altitude on each side of the meridian, we ought to make one observation when the hour-angle is as large as possible, and with this all the rest should be separately combined. We must be cautious, however, not to let the sun be too near the horizon, lest the apparent altitude should be affected by the uncertainty of the refraction.

3. If the clock were to furnish us with the true time, we might combine together any two observations made within the proper limits, without applying to the first table, and deduce a very exact correction. Should there even be a small error in the supposed time, we might still proceed in the same manner, without being liable to any material inaccuracy, provided the difference betwixt the hour-angles was not very considerable. The error, indeed, occasioned by adopting this method of finding the two areas, and taking a mean betwixt them for the value of $gb$, may easily be determined in any particular case. If $t$ and $t'$ be the respective tangents of the smaller and greater hour-angles, and $\dot{t}$ their difference; $\dot{z}$ the error of the clock in

minutes of time; and $\dot{Z}$ the whole error in the time computed with the greater altitude; then will the error in the result be to the whole correction of the latitude $(\dot{L}) :: \frac{t'\dot{z} - t\dot{z}}{2} : t\dot{z} :: \frac{\dot{t}\dot{z}}{2} : \frac{t\dot{L}}{15 T\lambda} :: \frac{\dot{t}\dot{z}}{2} : \frac{l\gamma\delta - d\lambda}{15\lambda\gamma\delta} \times \dot{L} :: 7{,}5 \times \dot{t}\dot{z} : \overline{s - \frac{r}{\gamma}}\, \dot{L}$. This error will consequently be equal to $\frac{7{,}5\, \dot{t}\dot{z}}{s - \frac{r}{\gamma}}$; and hence it appears that, if we had pursued this method in the last example, and there had been an error of a minute in the time given by the clock, there would not have been an error of a single second in the conclusion.

4. If the time were determined by equal altitudes, and one of them were to be employed in computing the area *g b*, it is manifest that we should entirely exclude the error which has just been considered. It would be necessary, however, in order to correct by the second observation any inaccuracy that may have occurred in reading off at the first, to move the index, and then bring it apparently to the same position again, before we proceeded to take the second altitude.

### EXAMPLE V.

On the 30th of January, 1799, the following altitudes of the sun's lower limb were taken in Trinity College, Cambridge; the height of the barometer being 29,6 inches, and of the thermometer 31 degrees.

| Observed double alt. by the sextant. | Time by the clock. | Time from noon by the sun. | Log. cos. of the hour-angle. | Lat. from each pair of corresponding altitudes. |
|---|---|---|---|---|
| 39° 35′ 10″ | 11ʰ 21′ 0″ | 23′ 40″ | 9,9976800 | 52° 12′ 39″ |
| 40 7 | 20 51 | 20 49 | 8206 | 46 |
| 44 55 | 26 49 | 17 51 | 8681 | 45 |
| 48 5 | 29 13 | 15 27 | 9012 | 44 |
| 50 26 | 31 1 | 13 39 | 9229 | 40 |
| 52 30 | 33 0 | 11 40 | 9437 | 41 |
| 53 40 | 34 35 | 10 5 | 9579 | 41 |
| 55 19 | 36 46 | 7 54 | 9742 | 43 |
| 56 50 | 38 56 | 5 44 | 9864 | 38 |
| 57 20 | 41 4 | 3 36 | 9946 | 41 |
| 57 55 | 43 54 | 0 46 | 9999 | 41 |
| 57 50 | 46 40 | — 2 0 | 9983 | 41 |
| 57 28 | 48 12 | 3 32 | 9948 | 41 |
| 57 0 | 49 59 | 5 19 | 9883 | 38 |
| 56 0 | 51 31 | 6 51 | 9806 | 43 |
| 55 15 | 53 0 | 8 20 | 9713 | 41 |
| 53 26 | 55 2 | 10 22 | 9556 | 41 |
| 51 5 | 57 44 | 13 4 | 9295 | 40 |
| 48 38 | 59 46 | 15 6 | 9057 | 44 |
| 45 17 | 12 2 9 | 17 29 | 8735 | 45 |
| 41 50 | 4 24 | 19 44 | 8388 | 46 |
| 35 10 | 8 20 | 23 40 | 7680 | 39 |
| 22) 1102 556 | | | 22) ,2034190 | |
| 39 50 31 | | | 9,9992463 | |

| | | | |
|---|---|---|---|
| | 39 | 50 | 31 |
| Error of adjust. | - | - | 48 |
| 2) | 39 | 51 | 19 |
| | 19 | 55 | 39 |
| Refra. — parall. | | 2 | 35 |
| | 19 | 53 | 4 |
| Semi-diameter | | 16 | 18 |
| Assumed merid. alt. | 20 | 9 | 22 |
| Mean declin. | 17 | 34 | 13 |
| | 37 | 43 | 35 |
| Assumed lat. | 52 | 16 | 25 |
| Correction from tab. 2d. | | 3 | 43 |
| True lat. - | 52 | 12 | 42 |

Area $gb =$ 7537
Log. $gb =$ 3,8772

Const. log. + log. rad. 13,1015
Log. cos. merid. alt. 9,9726
23,0741
— Declin. 9,9793
— Lat. 9,7867
Log. $gc =$ 3,3081
Log. $gb$ — log. $gc =$ ,5691

True lat. 52 12 42 not less accurate than the mean result from 22 meridian altitudes.

This process will require very little explanation. The first column contains the observed double altitudes, as they were read off from the sextant; the second contains the corresponding times given by the clock; the third contains the times from noon determined, with sufficient exactness, by taking half the interval betwixt the first and last observations, (in which the altitudes are equal,) and supposing this to be the time elapsed betwixt the first observation and the sun's reaching the meridian. The fourth column is formed of the log. cosines of the hour-angles taken immediately from the argument of the first table; and the last column exhibits the latitudes deduced from every two corresponding altitudes, and is intended to shew the agreement betwixt these results.

It is not necessary that we should employ the tables in this operation: for we may take the log. cosines of the hour-angles from TAYLOR's Logarithms, after the time is reduced into degrees, minutes, and seconds; and, by dividing the area $gb = 7537$, by the area $gc = 203$, (the natural number belonging to the log. 3,3081,) we shall obtain the correction required.

# TABLE I.

*To face page 116.*

| | | 0′ 12″ | 0′ 24″ | 0′ 36″ | 0′ 48″ | 1′ 0″ | 1′ 12″ | 1′ 24″ | 1′ 36″ | 1′ 48″ | 2′ 0″ | 2′ 12″ | 2′ 24″ | 2′ 36″ | 2′ 48″ | 3′ 0″ | 3′ 12″ | 3′ 24″ | 3′ 36″ | 3′ 48″ | 4′ 0″ | 4′ 12″ |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 9.99 | 2 . 2 | 2 . 2 | 2 . 2 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 | 3 . 3 |
| 16′ 0″ | 8941 | 43.42 | 73.72 | 908.892 | 036.014 | 136.108 | 217.185 | 287.249 | 348.304 | 401.352 | 450.395 | 494.434 | 534.468 | 571.500 | 606.530 | 638.556 | 669.581 | 698.605 | 725.627 | 751.647 | 776.666 | 799.684 |
| — 8 | 923 | 43.42 | 73.72 | 912.896 | 039.018 | 139.112 | 221.189 | 290.253 | 351.308 | 405.356 | 453.399 | 497.438 | 537.472 | 575.504 | 609.534 | 642.561 | 672.586 | 701.609 | 728.631 | 754.651 | 779.670 | 802.688 |
| — 16 | 905 | 43.43 | 74.73 | 916.899 | 043.022 | 143.116 | 225.192 | 294.256 | 354.312 | 408.360 | 457.403 | 501.441 | 541.476 | 578.508 | 613.537 | 645.564 | 676.590 | 704.613 | 732.635 | 757.655 | 782.674 | 806.692 |
| — 24 | 887 | 44.43 | 74.73 | 919.903 | 046.025 | 146.120 | 228.196 | 297.260 | 358.315 | 411.364 | 460.407 | 504.445 | 544.480 | 581.512 | 616.541 | 648.568 | 679.594 | 707.617 | 735.639 | 761.659 | 785.678 | 809.697 |
| — 32 | 869 | 44.44 | 74.73 | 922.907 | 050.029 | 150.123 | 231.200 | 301.264 | 361.319 | 415.368 | 463.410 | 507.449 | 547.484 | 584.516 | 619.545 | 651.572 | 682.597 | 711.621 | 738.643 | 764.663 | 788.683 | 812.701 |
| — 40 | 850 | 44.44 | 75.74 | 926.910 | 053.033 | 153.127 | 235.203 | 304.267 | 365.323 | 418.371 | 466.414 | 510.453 | 551.488 | 588.520 | 622.549 | 655.576 | 685.601 | 714.625 | 741.647 | 767.667 | 792.686 | 815.705 |
| — 48 | 832 | 45.44 | 75.74 | 929.914 | 057.036 | 156.130 | 238.207 | 307.271 | 368.326 | 421.375 | 470.418 | 514.457 | 554.492 | 591.523 | 626.553 | 658.580 | 688.605 | 717.628 | 744.651 | 770.671 | 795.690 | 818.709 |
| — 56 | 813 | 45.45 | 75.74 | 933.918 | 060.039 | 160.134 | 241.210 | 311.275 | 371.330 | 425.378 | 473.421 | 517.460 | 557.495 | 594.527 | 629.557 | 661.584 | 692.608 | 720.632 | 747.654 | 773.675 | 798.694 | 821.712 |
| 17 4 | 795 | 45.45 | 76.75 | 936.921 | 063.043 | 163.137 | 245.214 | 314.278 | 374.334 | 428.382 | 476.425 | 520.464 | 560.499 | 597.531 | 632.560 | 664.587 | 695.613 | 723.636 | 750.658 | 776.679 | 801.698 | 824.716 |
| — 12 | 776 | 46.45 | 76.75 | 940.924 | 067.046 | 166.141 | 248.217 | 317.282 | 378.337 | 431.386 | 479.429 | 523.467 | 563.502 | 601.535 | 635.564 | 667.591 | 698.616 | 726.640 | 754.662 | 779.683 | 804.702 | 827.720 |
| — 20 | 757 | 46.46 | 76.75 | 943.928 | 070.050 | 169.145 | 251.221 | 321.285 | 381.341 | 434.389 | 483.432 | 526.471 | 567.506 | 604.538 | 638.568 | 670.595 | 701.620 | 729.644 | 756.666 | 782.686 | 807.706 | 830.724 |
| — 28 | 737 | 46.46 | 77.76 | 946.931 | 073.053 | 173.148 | 254.225 | 324.289 | 384.344 | 438.393 | 486.436 | 530.475 | 570.510 | 607.542 | 641.571 | 673.598 | 704.624 | 732.647 | 760.669 | 785.690 | 810.710 | 833.728 |
| — 36 | 718 | 47.46 | 77.76 | 949.934 | 077.057 | 176.151 | 257.228 | 327.292 | 387.348 | 441.396 | 489.439 | 533.478 | 573.513 | 610.545 | 644.575 | 677.602 | 707.627 | 735.651 | 763.673 | 788.694 | 813.713 | 836.732 |
| — 44 | 699 | 47.47 | 77.76 | 952.938 | 080.060 | 179.155 | 261.231 | 330.296 | 390.351 | 444.400 | 492.443 | 536.482 | 576.517 | 613.549 | 647.578 | 680.606 | 710.631 | 738.655 | 766.677 | 791.697 | 816.717 | 839.735 |
| — 52 | 679 | 47.47 | 78.77 | 956.941 | 083.063 | 182.158 | 264.235 | 333.299 | 394.354 | 447.403 | 495.446 | 539.485 | 579.520 | 616.552 | 651.582 | 683.609 | 713.635 | 742.658 | 769.680 | 794.701 | 819.721 | 842.739 |
| 18 0 | 659 | 48.47 | 78.77 | 959.944 | 086.067 | 186.161 | 267.238 | 336.303 | 397.358 | 450.407 | 498.450 | 542.489 | 582.524 | 619.556 | 653.585 | 686.613 | 716.638 | 744.662 | 771.684 | 797.705 | 822.724 | 845.743 |
| — 8 | 639 | 48.48 | 78.77 | 962.947 | 090.070 | 189.165 | 270.241 | 339.306 | 400.361 | 453.410 | 501.453 | 545.492 | 585.527 | 622.559 | 656.589 | 689.616 | 719.642 | 747.665 | 774.688 | 800.708 | 825.728 | 848.746 |
| — 16 | 619 | 48.48 | 79.78 | 965.951 | 093.073 | 192.168 | 273.245 | 343.309 | 403.365 | 456.413 | 504.457 | 548.495 | 588.531 | 625.563 | 659.592 | 692.620 | 722.645 | 750.669 | 777.691 | 803.712 | 827.732 | 851.750 |
| — 24 | 599 | 49.48 | 79.78 | 968.954 | 096.077 | 195.171 | 276.248 | 346.312 | 406.368 | 459.417 | 507.460 | 551.499 | 591.534 | 628.566 | 662.596 | 694.623 | 725.649 | 753.672 | 780.695 | 806.716 | 830.735 | 854.754 |
| — 32 | 478 | 49.49 | 79.78 | 971.958 | 099.080 | 198.174 | 279.251 | 349.316 | 409.371 | 462.420 | 510.463 | 554.502 | 594.537 | 631.570 | 665.599 | 697.627 | 728.652 | 756.676 | 783.698 | 809.719 | 833.739 | 856.757 |
| — 40 | 558 | 49.49 | 80.79 | 975.960 | 102.083 | 201.178 | 282.254 | 352.319 | 412.374 | 465.423 | 513.466 | 557.505 | 597.541 | 634.573 | 668.603 | 700.630 | 731.656 | 759.679 | 786.701 | 812.722 | 836.742 | 859.761 |
| — 48 | 537 | 50.49 | 80.79 | 978.964 | 105.086 | 204.181 | 286.257 | 355.322 | 415.378 | 468.426 | 516.470 | 560.509 | 600.544 | 637.576 | 671.606 | 703.633 | 733.659 | 762.683 | 789.705 | 814.726 | 839.746 | 862.764 |
| — 56 | 516 | 50.49 | 80.79 | 980.967 | 108.090 | 207.184 | 288.261 | 358.325 | 418.381 | 471.430 | 519.473 | 563.512 | 603.547 | 640.580 | 674.609 | 706.637 | 736.662 | 765.686 | 792.708 | 817.729 | 842.749 | 865.768 |
| 19 4 | 495 | 50.50 | 80.80 | 984.970 | 111.093 | 210.187 | 291.264 | 360.329 | 421.384 | 474.433 | 522.476 | 566.515 | 606.551 | 642.583 | 677.613 | 709.640 | 739.666 | 767.689 | 794.712 | 820.733 | 844.752 | 868.771 |
| — 12 | 474 | 51.50 | 81.80 | 987.973 | 114.096 | 213.190 | 294.267 | 363.332 | 424.387 | 477.436 | 525.479 | 568.518 | 608.554 | 645.586 | 680.616 | 712.643 | 742.669 | 770.693 | 797.715 | 823.736 | 847.756 | 870.774 |
| — 20 | 453 | 51.50 | 81.80 | 989.976 | 117.099 | 216.193 | 297.270 | 366.335 | 427.390 | 480.439 | 528.483 | 571.522 | 611.557 | 648.589 | 682.619 | 714.647 | 745.672 | 773.696 | 800.718 | 825.739 | 850.759 | 873.778 |
| — 28 | 431 | 51.51 | 81.80 | 993.979 | 120.102 | 219.196 | 300.273 | 369.338 | 430.394 | 483.442 | 531.486 | 574.525 | 614.560 | 651.593 | 685.622 | 717.650 | 747.676 | 776.699 | 803.722 | 828.743 | 853.763 | 876.781 |
| — 36 | 410 | 51.51 | 82.81 | 996.982 | 123.105 | 222.199 | 303.276 | 372.341 | 432.397 | 486.445 | 534.489 | 577.528 | 617.563 | 654.596 | 688.626 | 720.653 | 750.679 | 779.703 | 805.725 | 831.746 | 855.766 | 878.785 |
| — 44 | 388 | 52.51 | 82.81 | 998.985 | 125.109 | 225.202 | 306.279 | 375.344 | 435.400 | 488.449 | 536.492 | 580.531 | 620.567 | 656.599 | 691.629 | 723.656 | 753.682 | 781.706 | 808.728 | 834.749 | 858.769 | 881.788 |
| — 52 | 366 | 52.51 | 82.81 | 001.988 | 129.111 | 228.205 | 309.282 | 378.347 | 438.403 | 491.452 | 539.495 | 583.534 | 623.570 | 659.602 | 694.632 | 726.659 | 756.685 | 784.709 | 811.732 | 836.753 | 861.772 | 884.791 |
| 20 0 | 344 | 52.52 | 83.82 | 004.991 | 131.114 | 230.208 | 312.286 | 381.350 | 441.406 | 494.455 | 542.498 | 585.537 | 625.573 | 662.605 | 696.635 | 728.663 | 758.688 | 787.712 | 814.735 | 839.756 | 863.776 | 886.794 |
| — 8 | 322 | 53.52 | 83.82 | 007.994 | 134.117 | 233.212 | 314.288 | 383.353 | 444.409 | 497.458 | 545.501 | 588.540 | 628.576 | 665.608 | 699.638 | 731.666 | 761.692 | 789.716 | 816.738 | 842.759 | 866.779 | 889.798 |
| — 16 | 300 | 53.53 | 83.82 | 010.997 | 137.120 | 236.215 | 317.291 | 386.356 | 446.412 | 500.461 | 547.504 | 591.543 | 631.579 | 667.611 | 702.641 | 734.669 | 764.695 | 792.719 | 819.741 | 844.762 | 869.782 | 892.801 |
| — 24 | 277 | 53.53 | 83.83 | 012.000 | 140.123 | 239.217 | 320.294 | 389.359 | 449.415 | 502.464 | 550.507 | 594.547 | 633.582 | 670.614 | 704.644 | 736.672 | 766.698 | 795.722 | 821.744 | 847.765 | 871.785 | 894.804 |
| — 32 | 255 | 53.53 | 84.83 | 015.003 | 142.125 | 242.220 | 323.297 | 392.362 | 452.418 | 505.467 | 553.510 | 596.549 | 636.585 | 673.618 | 707.647 | 739.675 | 769. 01 | 797.725 | 824.747 | 849.768 | 874.788 | 897.807 |
| — 40 | 232 | 54.53 | 84.83 | 018.006 | 145.129 | 244.223 | 326.300 | 394.365 | 455.421 | 508.470 | 555.513 | 599.552 | 639.588 | 675.621 | 710.651 | 742.678 | 772. 04 | 800.728 | 827.750 | 852.772 | 876.792 | 899.810 |
| — 48 | 209 | 54.54 | 84.83 | 021.009 | 148.131 | 247.226 | 328.303 | 397.368 | 457.424 | 510.473 | 558.516 | 602.555 | 641.591 | 678.624 | 712.653 | 744.681 | 774. 07 | 803.731 | 829.754 | 855.775 | 879.795 | 902.814 |
| — 56 | 186 | 54.54 | 85.84 | 024.011 | 151.134 | 250.229 | 331.306 | 400.371 | 460.427 | 513.476 | 561.519 | 604.558 | 644.594 | 681.626 | 715.656 | 747.684 | 777. 10 | 805.734 | 832.756 | 857.778 | 881.798 | 904.817 |
| 21 2 | 163 | 55.54 | 85.84 | 027.015 | 154.137 | 252.232 | 334.309 | 403.374 | 463.430 | 516.479 | 563.522 | 607.561 | 647.597 | 683.629 | 718.659 | 749.687 | 779. 13 | 808.737 | 834.760 | 860.781 | 884.801 | 907.820 |
| — 12 | 139 | 55.54 | 85.84 | 029.017 | 156.140 | 255.235 | 336.312 | 405.376 | 465.432 | 519.481 | 566.525 | 610.564 | 649.600 | 686.632 | 720.662 | 752.690 | 782. 16 | 810.740 | 837.763 | 862.784 | 886.804 | 910.823 |
| — 20 | 116 | 55.55 | 85.85 | 032.020 | 159.142 | 258.237 | 339.314 | 408.379 | 468.435 | 521.484 | 569.528 | 612.567 | 652.603 | 689.635 | 723.665 | 755.693 | 785. 19 | 813.743 | 839.766 | 865.787 | 889.807 | 912.826 |
| — 28 | 092 | 55.55 | 86.85 | 035.022 | 162.145 | 261.240 | 342.317 | 411.382 | 471.438 | 524.487 | 571.531 | 615.570 | 654.606 | 691.638 | 725.668 | 757.696 | 787. 22 | 815.746 | 842.769 | 867.790 | 891.810 | 915.829 |
| — 36 | 068 | 56.55 | 86.85 | 037.025 | 164.148 | 263.243 | 344.320 | 413.385 | 473.441 | 526.490 | 574.534 | 617.573 | 657.608 | 694.641 | 728.671 | 760.699 | 790. 25 | 818.749 | 844.771 | 870.793 | 894 813 | 917.832 |
| — 44 | 044 | 56.56 | 86.85 | 040.028 | 167.151 | 266.246 | 347.323 | 416.388 | 476.444 | 529.493 | 576.536 | 620.576 | 660.611 | 696. 44 | 730.674 | 762.702 | 792. 28 | 820.752 | 847.774 | 872.796 | 896.816 | 919 835 |
| — 52 | 020 | 56.56 | 86.86 | 043.031 | 169.154 | 268.248 | 349.326 | 418.391 | 478.446 | 531.496 | 579.539 | 622.578 | 662.614 | 699. 47 | 733.677 | 765.705 | 795. 31 | 823.755 | 849.777 | 875.799 | 899.819 | 922.838 |
| 22 0 | 7996 | 56.56 | 87.86 | 045.033 | 172.156 | 271.251 | 352.328 | 421.393 | 481.449 | 534.498 | 582.542 | 625.581 | 665.617 | 701.650 | 735.680 | 767.707 | 797. 33 | 825.758 | 852.780 | 877.802 | 901.822 | 924.841 |
| — 8 | 972 | 57.56 | 87.86 | 048.036 | 175.159 | 274.254 | 355.331 | 424.396 | 483.452 | 537.501 | 584.545 | 627.584 | 667.620 | 704.652 | 738.682 | 770.710 | 799. 36 | 828.760 | 854 783 | 880.804 | 904.825 | 927.844 |
| — 16 | 947 | 57.57 | 87.86 | 050.039 | 177.162 | 276.256 | 357.334 | 426.399 | 486 455 | 539.504 | 587.547 | 630.587 | 670 623 | 706.655 | 740.685 | 772.713 | 802.739 | 830.763 | 857.786 | 882.807 | 906.827 | 929.846 |
| — 24 | 922 | 57.57 | 88.87 | 053.041 | 180.164 | 279.259 | 360.336 | 429.401 | 489 457 | 542.507 | 589.550 | 632.590 | 672.625 | 709.658 | 743.688 | 775.716 | 804.742 | 833.766 | 859.789 | 884.810 | 909.830 | 932.849 |
| — 32 | 910 | 58.57 | 88.87 | 056.044 | 182.167 | 281.262 | 362.339 | 431.404 | 491.460 | 544.509 | 592.553 | 635.592 | 675.628 | 711.661 | 745.691 | 777.719 | 807.745 | 835.769 | 862.792 | 887.813 | 911.833 | 934.852 |
| — 40 | 885 | 58.57 | 88.87 | 058.046 | 185.169 | 284.264 | 365.342 | 434.407 | 494 463 | 547.512 | 594.555 | 637.595 | 677.631 | 714.663 | 748.694 | 779.721 | 809.747 | 837.772 | 864 794 | 889.816 | 913 836 | 936.855 |
| — 48 | 860 | 58.58 | 88.88 | 061.049 | 188.172 | 286.267 | 367.344 | 436.409 | 496.465 | 549.515 | 597.558 | 640.598 | 679.633 | 716.666 | 750.696 | 782.724 | 812.750 | 840.774 | 866.797 | 892.819 | 916.839 | 939.858 |
| — 56 | 835 | 58.58 | 88.88 | 063.052 | 190.175 | 289.270 | 370.347 | 439.412 | 498.468 | 551.517 | 599 561 | 642.600 | 682.636 | 718.669 | 752.699 | 784.727 | 814.753 | 842.777 | 869.800 | 894.821 | 918.842 | 941.861 |
| 23 4 | 809 | 58.58 | 89.88 | 066.054 | 193.177 | 291.272 | 372.349 | 441.414 | 501.471 | 554.520 | 602.563 | 645.603 | 684.639 | 721 672 | 755.702 | 787.730 | 816.756 | 844.780 | 871.803 | 896.824 | 920.844 | 943.863 |
| — 12 | 784 | 59.58 | 89.88 | 068.057 | 195.180 | 294.275 | 375.352 | 444.417 | 503.473 | 556.522 | 604.566 | 647.606 | 687.641 | 723.674 | 757.704 | 789.732 | 819.758 | 847.783 | 873.805 | 899.827 | 923.847 | 946.866 |
| — 20 | 758 | 59.59 | 89.88 | 071.059 | 198.182 | 296.277 | 377.355 | 446.420 | 506.476 | 559.525 | 606.569 | 650.608 | 689.644 | 726.677 | 760.707 | 791.735 | 821.761 | 849.785 | 876.808 | 901.830 | 925 850 | 948.869 |
| — 28 | 732 | 59.59 | 90.89 | 073.062 | 200.185 | 299.280 | 380.357 | 448 422 | 508.478 | 561.528 | 609.571 | 652.611 | 691.647 | 728.679 | 762.710 | 794.738 | 823.764 | 852.788 | 878.811 | 903.832 | 927.853 | 950.872 |
| — 36 | 706 | 59.59 | 90.89 | 076.064 | 202.188 | 301.281 | 382.360 | 451.425 | 511.481 | 563.530 | 611.574 | 654.614 | 694.649 | 730.682 | 764.712 | 796 740 | 826.766 | 854.791 | 880.814 | 906 835 | 930.855 | 952.874 |
| — 44 | 680 | 60 59 | 90.89 | 078.067 | 205.190 | 304.285 | 385.362 | 453.427 | 513.483 | 566.533 | 613.576 | 657.616 | 696.652 | 733.685 | 767.715 | 798.743 | 828.769 | 856.793 | 883.816 | 908.838 | 932.858 | 955.877 |
| — 52 | 654 | 60 60 | 90.90 | 080.070 | 207.193 | 306.288 | 387.365 | 455.430 | 515.486 | 568.535 | 616.579 | 659.619 | 698.654 | 735.687 | 769.718 | 801.746 | 830.772 | 858.796 | 885.819 | 910.840 | 934.861 | 957 880 |

TAB. II.

| | | |
|---|---|---|
| — 1,176 | 0′ | 4″ |
| — ,875 | 0 | 8 |
| — ,699 | 0 | 12 |
| — ,577 | 0 | 16 |
| — ,477 | 0 | 20 |
| — ,399 | 0 | 24 |
| — ,331 | 0 | 28 |
| — ,273 | 0 | 32 |
| — ,222 | 0 | 36 |
| — ,176 | 0 | 40 |
| — ,134 | 0 | 44 |
| — ,097 | 0 | 48 |
| — ,062 | 0 | 52 |
| — ,030 | 0 | 56 |
| ,000 | 1 | 0 |
| ,028 | 1 | 4 |
| ,054 | 1 | 8 |
| ,079 | 1 | 12 |
| ,103 | 1 | 16 |
| ,125 | 1 | 20 |
| ,146 | 1 | 24 |
| ,166 | 1 | 28 |
| ,186 | 1 | 32 |
| ,204 | 1 | 36 |
| ,222 | 1 | 40 |
| ,239 | 1 | 44 |
| ,255 | 1 | 48 |
| ,271 | 1 | 52 |
| ,286 | 1 | 56 |
| ,301 | 2 | 0 |

TAB. III.

| h. | ′. | ° | ′ | Log. of *g* or *α*. | Log. of *g*². |
|---|---|---|---|---|---|
| 0 | 1 | 89 | 45 | 7,64 | 15,28 |
| 0 | 2 | 89 | 30 | 7,94 | 15,88 |
| 0 | 3 | 89 | 15 | 8,12 | 16,24 |
| 0 | 4 | 89 | 0 | 8,24 | 16,48 |
| 0 | 5 | 88 | 45 | 8,34 | 16,68 |
| 0 | 6 | 88 | 30 | 8,42 | 16,84 |
| 0 | 7 | 88 | 15 | 8,48 | 16,96 |
| 0 | 8 | 88 | 0 | 8,54 | 17,08 |
| 0 | 9 | 87 | 45 | 8,59 | 17,18 |
| 0 | 10 | 87 | 30 | 8,64 | 17,28 |
| 0 | 11 | 87 | 15 | 8,68 | 17,36 |
| 0 | 12 | 87 | 0 | 8,72 | 17,44 |
| 0 | 13 | 86 | 45 | 8,75 | 17,50 |
| 0 | 14 | 86 | 30 | 8,78 | 17,56 |
| 0 | 15 | 86 | 15 | 8,81 | 17,62 |
| 0 | 16 | 86 | 0 | 8,84 | 17,68 |
| 0 | 17 | 85 | 45 | 8,87 | 17,74 |
| 0 | 18 | 85 | 30 | 8,89 | 17,78 |
| 0 | 19 | 85 | 15 | 8,92 | 17,84 |
| 0 | 20 | 85 | 0 | 8,94 | 17,88 |
| 0 | 22 | 84 | 30 | 8,98 | 17,96 |
| 0 | 24 | 84 | 0 | 9,02 | 18,04 |
| 0 | 26 | 83 | 30 | 9,05 | 18,10 |
| 0 | 28 | 83 | 0 | 9,08 | 18,16 |
| 0 | 30 | 82 | 30 | 9,11 | 18,22 |
| 0 | 32 | 82 | 0 | 9,14 | 18,28 |
| 0 | 34 | 81 | 30 | 9,17 | 18,34 |

TAB. IV.

| | 10° | 20° | 30° | 35° | 40° | 45° | 50° | 55° | 60° |
|---|---|---|---|---|---|---|---|---|---|
| 8,64 | ,64 | ,61 | ,57 | ,54 | ,50 | ,46 | ,42 | ,38 | ,33 |
| 8,68 | ,70 | ,67 | ,69 | ,59 | ,55 | ,51 | ,46 | ,41 | ,36 |
| 8,71 | ,77 | ,74 | ,68 | ,64 | ,60 | ,55 | ,50 | ,45 | ,39 |
| 8,75 | ,84 | ,80 | ,74 | ,70 | ,65 | ,60 | ,55 | ,49 | ,43 |
| 8,78 | ,90 | ,86 | ,79 | ,75 | ,70 | ,65 | ,59 | ,53 | ,46 |
| 8,81 | ,97 | ,92 | ,85 | ,81 | ,75 | ,69 | ,63 | ,56 | ,49 |
| 8,84 | 1, 0 | ,98 | ,91 | ,86 | ,80 | ,74 | ,67 | ,60 | ,52 |
| 8,87 | 1, 1 | 1, 0 | ,96 | ,91 | ,85 | ,79 | ,71 | ,64 | ,56 |
| 8,89 | 1, 2 | 1, 1 | 1, 0 | ,97 | ,90 | ,83 | ,76 | ,68 | ,59 |
| 8,92 | 1, 2 | 1, 2 | 1, 1 | 1, 0 | ,95 | ,88 | ,80 | ,71 | ,62 |
| 8,94 | 1, 3 | 1, 2 | 1, 1 | 1, 1 | 1, 0 | ,93 | ,84 | ,75 | ,66 |
| 8,98 | 1, 4 | 1, 3 | 1, 2 | 1, 2 | 1, 1 | 1, 0 | ,93 | ,83 | ,72 |
| 9,02 | 1, 5 | 1, 5 | 1, 4 | 1, 3 | 1, 2 | 1, 1 | 1, 0 | ,90 | ,79 |
| 9,05 | 1, 7 | 1, 6 | 1, 5 | 1, 4 | 1, 3 | 1, 2 | 1, 1 | ,98 | ,86 |
| 9,09 | 1, 8 | 1, 7 | 1, 6 | 1, 5 | 1, 4 | 1, 3 | 1, 2 | 1, 1 | ,92 |

Tab. V.

| | 10° | 20° | 30° | 35° | 40° | 45° | 50° | 55° | 60° |
|---|---|---|---|---|---|---|---|---|---|
| | 89° | 89° | 89° | 89° | 89° | 89° | 89° | 89° | 89° |
| 6,463 | 59′ 30″ | 59′ 32″ | 59′ 34″ | 59′ 36″ | 59′ 38″ | 59′ 40″ | 59′ 42″ | 59′ 44″ | 59′ 46″ |
| 6,765 | — 0 | — 4 | — 8 | — 11 | — 14 | — 18 | — 21 | — 26 | — 30 |
| 6,941 | 58 30 | 58 35 | 58 42 | 58 46 | 58 51 | 58 56 | — 2 | — 8 | — 15 |
| 7,066 | — 0 | — 7 | — 14 | — 22 | — 28 | — 35 | 58 43 | 58 51 | 58 59 |
| 7,163 | 57 30 | 57 38 | 57 49 | 57 57 | — 5 | — 14 | — 23 | — 33 | — 45 |
| 7,242 | — 0 | — 11 | — 24 | — 33 | 57 42 | 57 53 | — 4 | — 17 | — 30 |
| 7,309 | 56 30 | 56 42 | 56 58 | — 8 | — 19 | — 32 | 57 45 | 57 59 | — 12 |
| 7,367 | — 0 | — 14 | — 32 | 56 43 | 56 56 | — 10 | — 25 | — 42 | — 0 |
| 7,418 | 55 30 | 55 46 | — 6 | — 19 | — 34 | 56 50 | — 6 | — 25 | 57 45 |
| 7,464 | — 0 | — 18 | 55 40 | 55 54 | — 10 | — 28 | 56 47 | — 7 | — 30 |
| 7,505 | 54 30 | 54 49 | — 14 | — 39 | 55 47 | — 7 | — 28 | 56 48 | — 15 |
| 7,543 | — 0 | — 21 | 54 48 | — 5 | — 14 | 55 45 | — 8 | — 33 | 56 59 |
| 7,578 | 53 30 | 53 50 | — 22 | 54 40 | — 1 | — 14 | 55 49 | — 16 | — 45 |
| 7,610 | — 0 | — 25 | 53 56 | — 16 | 54 38 | — 3 | — 30 | 55 59 | — 29 |
| 7,640 | 52 30 | 52 57 | — 30 | 53 51 | — 15 | 54 42 | — 11 | — 41 | — 15 |

## EXPLANATION OF THE TABLES.

Tab. I. This table contains two arguments. One is the distance of the sun from the meridian, which is expressed in hours, minutes, and seconds; and also in terms of the log. cosine of the hour-angle. This argument is placed in the first column. The second stands at the top of the table, and consists of the minutes and seconds contained in the error of time. The table itself exhibits the logarithm of the area *gb* corresponding to any given error at any particular observation. But it was necessary to give the log. value of this area in both directions from the tangent *gd*, i. e. both when the computed is greater, and when it is less, than the real interval of time. These two values are included in the same column, but are separated from each other by a point. The first quantity expresses the log. area in the latter, the second expresses it in the former case. Thus, for instance, in the fifth column we have 136.108, and we are to understand that the log. of the area *gb* is equal to 136, when the computed time is too small; and equal to 108, when the computed time is too great.

The argument at the top is carried to every twelve seconds of time, or every three minutes of a great circle; and should be continued as far as four or five minutes of time for the first twelve or fourteen degrees from the meridian; as far as three minutes for the next ten degrees; two minutes for the next ten degrees; and so on, (the time being diminished as the tangent of the hour-angle increases,) as far as sixty or sixty-five degrees, beyond which the table need not be extended. The argument in the first column should be carried to every eight seconds of time for the first twelve or fourteen degrees; and to every twenty seconds for the rest of the table.

Tab. II. This table consists of two columns. The first contains the differences betwixt the logs. of the area *gb* and *gc*, when the latit. is varied one minute in calculating the incremental area. These differences are negative as far as the fourteenth, because so far the area *gc* exceeds the area *gb*. In the second column are exhibited the errors in the assumed latit. corresponding to these differences.

Tab. III. This table is composed of four columns. The first contains the computed distance in time from noon; the second the altitude of the sun; the third the log. sine of the hour-angle, or the log. cosine of the altitude, expressed in the first column; and the fourth twice the logarithm in the former column, being intended to save us the trouble of multiplying by 2, when we are finding the logarithm of $\frac{g^2}{a}$.

Tab. IV. In this table there are two arguments. One of them occupies the first column, and consists of the log. sines of the azimuth, which are found by subtracting the log. cosine of the altitude from the log. sine of the hour-angle, as they stand in the preceding table. The other argument is placed at the top of the columns, and is formed of the assumed latitude. The table is designed to exhibit the product of the tangent of the azimuth multiplied into the cosine of the latit. and into 15; so that, by taking out this quantity, and multiplying it into the minutes contained in the error of time, we may at once determine how much the assumed latit. is to be varied in computing the incremental area.

Tab. V. There are two arguments in this table likewise. That which stands in the first column is the log. value of $\frac{g^2}{a}$ obtained from the second table by subtracting the log. cosine of the altitude from twice the log. sine of the hour-angle. The second argument, which is placed above the columns, is the supposed latitude. The table itself contains the complement of the meridian altitude at the place of observation.

## VIII. *A Fourth Catalogue of the comparative Brightness of the Stars. By* William Herschel, *LL. D. F. R. S.*

Read February 21, 1799.

Lustre of the stars in Auriga.

| | | | |
|---|---|---|---|
| 1 | *f* | 5 | 2 , 1   4 , 1 |
| 2 | *g* | 5.6 | 4 , 2 , 1   2 . 4 |
| 3 | ι | 4 | 3 – 37   5 Arietis – 3 – 44 Persei   3 . 44 Persei |
| 4 | ω | 5 | 4 , 2   2 . 4 , 1 |
| 5 | | 6 | 5 – 6 |
| 6 | | 6 | 5 – 6   6 – 12 |
| 7 | ε | 4 | 7 – 10   7 –, 10 |
| 8 | ζ | 4 | 10 , 8   33 , 8   10 =, 8 –, 30 |
| 9 | | 6.5 | 7 Camelop ,̄ 9   11 Camelop , 9 –, 12 Camelop |
| 10 | η | 4 | 7 – 10 , 8   10 ,̄ 33   7 –, 10 =, 8 |
| 11 | μ | 5 | 15 ; 11 – 20   15 ,̄ 11 ,̄ 21 |
| 12 | | 6 | 6 – 12 |
| 13 | α | 1 | 13 – – 3 Lyræ |
| 14 | | 5 | 16 , 14 . 19 |
| 15 | λ | 5 | 15 ; 11   15 ,̄ 11 |
| 16 | | 6 | 16 , 14 |
| 17 | | 7.6 | 19 . 17 , 18 |
| 18 | | 8 | 17 , 18 |
| 19 | | 6 | 14 . 19 . 17 |
| 20 | ϱ | 6 | 11 – 20   21 . 20 |
| 21 | σ | 5.6 | 11 ,̄ 21 . 20 |
| 22 | | 6 | 26 , 22 |

Lustre of the stars in Auriga.

| | | | |
|---|---|---|---|
| 23 | γ | 2 | 13 Arietis , 23 · 34     34 ; 23     23 , 34 |
| 24 | φ | 5.6 | 25 ,̄ 24 |
| 25 | χ | 5.6 | 25 ,̄ 26     25 ,̄ 24 |
| 26 | | 6 | 25 ,̄ 26 , 22     27 , 26 |
| 27 | ο | 6 | 27 , 26 |
| 28 | | 7 | 40 – 28 |
| 29 | τ | 5 | 32 , 29 , 31     35 · 29 –, 36 |
| 30 | ξ | 6 | 8 –, 30     42 Camel . 30 – 31 Camel |
| 31 | υ | 6 | 29 , 31 · 40 |
| 32 | ν | 5 | 32 , 29 |
| 33 | δ | 4 | 10 ,̄ 33 , 8     33 ; 10 Camel |
| 34 | β | 2 | 23 · 34     66 Gemin . 34     34 : 23     23 , 34 |
| 35 | π | 6 | 35 · 29     35 , 46 |
| 36 | | 6 | 29 –, 36 |
| 37 | θ | 4 | 3 – 37     24 Gemin , 37 |
| 38 | | 6.7 | 39 · 38 , 42 |
| 39 | | 6.7 | 39 · 38 |
| 40 | | 6 | 31 . 40 – 28 |
| 41 | | 6 | 46 – 41 |
| 42 | | 6 | 38 , 42 · 43     47 , 42 · 43 |
| 43 | | 6 | 42 · 43 |
| 44 | κ | 4.5 | 44 , 136 Tauri     44 – – 49 |
| 45 | | 6 | 45 · 46 |
| 46 | | 5 | 35 , 46 – 41     45 · 46 |
| 47 | | 6 | 47 , 42 |
| 48 | | 6 | 49 –, 48 – 53 |
| 49 | | 5.6 | 44 – – 49 –, 48 |
| 50 | | 5.6 | 50 – 52     50 . 16 Lyncis |
| 51 | | 5.6 | 52 – 51 |
| 52 | | 5 | 50 – 52 – 51 |
| 53 | | 6 | 53 . 28 Gemin     48 – 53 , 54 |
| 54 | | 6 | 28 Gemin , 54 – 25 Gemin     53 , 54 |

### Lustre of the stars in Auriga.

| | | | |
|---|---|---|---|
| 55 | | 5 | 55 – 58 |
| 56 | | 6 | 58 , 56 . 57     16 Lyncis , 56 |
| 57 | | 6 | 56 . 57 |
| 58 | | 4.5 | 55 – 58 , 56 |
| 59 | | 6 | 62 . 59 . 60 |
| 60 | | 6 | 59 . 60 |
| 61 | | 6 | 61 . 62 |
| 62 | | 6.7 | 61 . 62 . 59 |
| 63 | | 4.5 | 63 . 65 |
| 64 | | 5 | 66 ; 64 |
| 65 | | 5 | 63 . 65 , 66 |
| 66 | | 5 | 65 , 66 ; 64 |

### Lustre of the stars in Draco.

| | | | |
|---|---|---|---|
| 1 | λ | 3.4 | 1 . 5 |
| 2 | | 6 | 5 – – 2     3 ; 2 |
| 3 | | 6 | 3 ; 2 |
| 4 | | 6 | 6 ⸵ 4 |
| 5 | κ | 3 | 1 . 5 – – 2     11 – 5 , 13 |
| 6 | | 6 | 6 ⸵ 4 |
| 7 | | 6 | 9 , 7 |
| 8 | | 6 | 10 – 8 , 9 |
| 9 | | 6 | 8 , 9 , 7 |
| 10 | *i* | 5 | 10 – 8 |
| 11 | α | 2 | 11 – 5 |
| 12 | ι | 3 | 40 Herculis – 12     22 –, 12     27 Herc – 12 , 67 Herculis |
| 13 | θ | 3 | 5 , 13 |
| 14 | η | 3 | 1 Ursæ min – 14     14 – – 22     14 – 23     14 –, 57 |
| 15 | A | 4 | 15 . 18 |
| 16 | | 5 | 17 – 16 |

### Lustre of the stars in Draco.

| | | | |
|---|---|---|---|
| 17 | | 5 | 17 – 16 |
| 18 | *g* | 5 | 19 , 18 –, 20    15 . 18    21 . 18 |
| 19 | | 5 | 19 , 18 |
| 20 | *h* | 6 | 18 –, 20 |
| 21 | $\mu$ | 5.4 | 21 . 18    30 , 21 |
| 22 | $\zeta$ | 2 | 14 – – 22 –, 12    57 $\overset{-}{,}$ 22 |
| 23 | $\beta$ | 2.3 | 23 , 40 Herculis    14 – 23    23 – 27 Herculis |
| 24 | $\nu^1$ | 4 | 24 . 25 |
| 25 | $\nu^2$ | 4 | 24 . 25 – 26 |
| 26 | | 6 | 25 – 26 |
| 27 | *f* | 5 | 28 – 27 , 34    27 . 42 |
| 28 | $\omega$ | 4 | 31 – 28 – 27 |
| 29 | | 6 | 34 – –, 29 |
| 30 | | 6 | 30 , 21 |
| 31 | $\psi^1$ | 7 | 31 – 28 |
| 32 | $\xi$ | 3 | 44 –, 32 – 43 |
| 33 | $\gamma$ | 2 | 7 Ursæ min – 33    5 Coronæ $\overset{-}{,}$ 33    55 Ophiu – 33 |
| 34 | $\psi^2$ | 4.5 | 27 , 34    35 · $\underline{34 - -,}$ 29    34 – 37 |
| 35 | | 6 | 35 · 34    35 – 40 + 41 |
| 36 | | 6 | 42 . 36 |
| 37 | | 6 | 34 – 37 , 38 |
| 38 | | 6 | 37 , 38 |
| 39 | *b* | 5 | 45 , 39 · 46 |
| 40 | | 5 | 41 – 40 |
| 41 | | 5 | 41 – 40 |
| 42 | | 6 | 27 . 42 . 36 |
| 43 | $\varphi$ | 5 | 32 – 43 |
| 44 | $\chi$ | 4 | 44 –, 32 |
| 45 | *d* | 5 | 45 , 39 |
| 46 | *c* | 5 | 39 · 46 ; 47 |

## Lustre of the stars in Draco.

| | | | |
|---|---|---|---|
| 47 | ο | 4 | 46 ; 47 − − 51    47 − 54 |
| 48 | | 6 | 49 . 48 |
| 49 | | 6 | 51 . 49 . 48 |
| 50 | | 4.5 | 52 . 50 ¯, 59    73 , 50    55 , 50    52 − 50 |
| 51 | | 5.6 | 47 − − 51 . 49 |
| 52 | υ | 4.5 | 60 − 52    52 . 50    52 − 50 |
| 53 | | 5 | 54 , 53 |
| 54 | | 5 | 47 − 54 , 53 |
| 55 | | 6 | 61 −, 55    55 , 50 |
| 56 | | 6 | Does not exist. |
| 57 | δ | 3.4 | 14 −, 57 ¯, 22 |
| 58 | π | 4 | 63 −, 58 − 61    58 ; 67 |
| 59 | | 6 | 50 ¯, 59 |
| 60 | τ | 4.5 | 60 − 52 |
| 61 | σ | 4.5 | 58 − 61 −, 55    67 , 61 |
| 62 | | 6 | Does not exist. |
| 63 | ε | 5.6 | 63 −, 58 |
| 64 | $e^1$ | 5.6 | 64 −, 65 |
| 65 | $e^2$ | 6.5 | 64 −, 65 −, 70    65 , 69 |
| 66 | | 6 | 68 . 66 . 71 |
| 67 | ϱ | 5 | 58 ; 67 , 61 |
| 68 | | 6 | 68 . 66 |
| 69 | | 6 | 65 , 69 |
| 70 | | 6 | 65 −, 70 |
| 71 | | 6 | 66 . 71 |
| 72 | | 6 | Does not exist. |
| 73 | | 5.6 | 73 , 50    73 − 77    73 , 78 |
| 74 | | 6 | 75 − 74 . 76 |
| 75 | | 6 | 75 − 74 |
| 76 | | 5 | 74 . 76 |
| 77 | | 5 | 73 − 77    78 , 77 |

## Lustre of the stars in Draco.

| | | | |
|---|---|---|---|
| 78 | | 5 | 73 , 78 , 77   16 Cephei , 78 |
| 79 | | 7 | 80 – 79 |
| 80 | | 6 | 80 – 79 |

## Lustre of the stars in Lynx.

| | | | |
|---|---|---|---|
| 1 | | 5.6 | 1 , 5 |
| 2 | | 4 | 2 – – 5 |
| 3 | | 6 | 8 – 3 , 10 |
| 4 | | 6 | 4 . 6 |
| 5 | | 6 | 2 – – 5 – 6   1 , 5 |
| 6 | | 6.7 | 5 – 6   4 . 6 |
| 7 | | 6.7 | Does not exist. |
| 8 | | 6.7 | 8 , 41 Camelop   8 – 3 |
| 9 | | 7 | 11 – 9 |
| 10 | | 6.7 | 41 Camelop – 10   3 , 10 |
| 11 | | 6 | 14 , 11 – 9 |
| 12 | | 7 | 15 ; 12 –, 14 |
| 13 | | 6 | 13 , 14 |
| 14 | | 5 | 12 –, 14   13 , 14 , 11   19 , 14 – 23 |
| 15 | | 5 | 15 ; 12 |
| 16 | | 6 | 50 Aurigæ . 16 , 56 Aurigæ |
| 17 | | 7 | 18 –, 17 |
| 18 | | 6 | 18 –, 47 Camelop   18 –, 17 |
| 19 | | 5 | 24 – 19 , 14 |
| 20 | | 6 | 21 – – 20 |
| 21 | | 5 | 22 – 21 – – 20 |
| 22 | | 6 | 22 – 21 |
| 23 | | 7 | 14 – 23 |
| 24 | | 5 | 24 – 19 |
| 25 | | 6 | 26 –, 25 |
| 26 | | 5 | 26 –, 25   26 , 34 |

### Lustre of the stars in Lynx.

| | | | |
|---|---|---|---|
| 27 | | 5 | 27 – 50 Camelop   31 –, 27 |
| 28 | | 7 | 36 – 28 |
| 29 | | 5 | 29 – 56 Camelop   29 , 58 Camelop   29 , 30 |
| 30 | | 6 | 29 , 30 |
| 31 | | 5 | 31 –, 27 |
| 32 | | 7 | 33 ; 32 |
| 33 | | 6 | 33 ; 32 |
| 34 | | 6 | 26 , 34   36 , 34 |
| 35 | | 7 | Does not exist |
| 36 | | 6 | 36 , 34   37 · 36 – 28 |
| 37 | | 5.6 | 37 · 36   37 , 42 |
| 38 | | 6 | 38 – 40 |
| 39 | | 4 | 12 Ursæ , 39   39 –, 10 Leonis min   39 , 10 Ursæ |
| 40 | | 6 | 38 – 40 |
| 41 | | 4 | 25 Ursæ . 41 – 12 Ursæ |
| 42 | | 6 | 37 , 42 · 45   15 Leonis min . 42 – – 14 Leonis min |
| 43 | | 6 | 43 – 44 |
| 44 | | 7.6 | 43 – 44 |
| 45 | | 5.6 | 29 Ursæ – – 45   42 · 45 |

### Lustre of the stars in Lyra.

| | | | |
|---|---|---|---|
| 1 | ϰ | 5 | 1 –, 2   6 – 1 |
| 2 | μ | 6 | 1 –, 2 |
| 3 | α | 1 | 16 Bootis – – 3 |
| 4 | ε | 5 | 6 ; 4 · 5 |
| 5 | | 6 | 4 · 5 |
| 6 | ζ | 5 | 6 – – 7   6 – 1   6 ; 4 |
| 7 | | 5 | 6 – – 7 |
| 8 | $\nu^1$ | 6 | 9 , 8 |
| 9 | $\nu^2$ | 6 | 11 · 9 , 8   15 , 9 , 8 |

## Lustre of the stars in Lyra.

| | | | |
|---|---|---|---|
| 10 | β | 3 | 10 . 14   14 $\overline{,}$ 10   14 -, 10   14 - - - 10<br>$\overline{6 + 7}$ $\underline{;}$ 10 |
| 11 | $\delta^1$ | 4.5 | 12 -, 11   18, 11 . 9   16, 11 |
| 12 | $\delta^2$ | 4 | 12 -, 11   12 . 13   12 - 18   12 - 16 |
| 13 | π | 6 | 12 . 13   20, 13 |
| 14 | γ | 3 | 6 Cygni -, 14, 85 Herculis |
| 15 | λ | 6 | 15, 9   15 . 17   15 $\overline{,}$ 19 |
| 16 | ρ | 6 | 12 - 16, 11 |
| 17 | | 6 | 15 . 17 |
| 18 | ι | 5 | 12 - 18, 11 |
| 19 | | 6 | 15 $\overline{,}$ 19 |
| 20 | η | 6 | 21 . 20, 13   20, 21 |
| 21 | θ | 6 | 21 . 20   20, 21 |

## Lustre of the stars in Monoceros.

| | | | |
|---|---|---|---|
| 1 | | 6 | 3 -, 1 -, 2 |
| 2 | | 6 | 1 -, 2, 4 |
| 3 | | 6 | 3 -, 1 |
| 4 | | 6 | 2, 4 . 6 |
| 5 | | 4.5 | 11 - 5 -, 10   5, 8 |
| 6 | | 6.7 | 4 . 6 |
| 7 | | 6 | 10 - 7 |
| 8 | | 4 | 5, 8 - 13   8 $\overline{,}$ 15   8, 18 |
| 9 | | 5 | 10 -, 9 |
| 10 | | 6 | 5 -, 10 -, 9   10 - 7 |
| 11 | | 5 | 22 Orionis - 11 - 5   30 ; 11 $\overline{,}$ 26 |
| 12 | | 5 | 13 -, 12, 14 |
| 13 | | 4 | 8 - 13 = $\overline{,}$ 14   13 -, 12 |
| 14 | | 5.6 | 13 = $\overline{,}$ 14   12, 14 |
| 15 | | 4 | 8 $\overline{,}$ 15, 17 |
| 16 | | 6 | 17 - 16 |

## Lustre of the stars in Monoceros.

| | | | |
|---|---|---|---|
| 17 | | 5 | 15 , 17 − 16 |
| 18 | | 4 | 8 , 18 |
| 19 | | 5 | 22 − 19 , 20 |
| 20 | | 6 | 19 , 20 , 25 |
| 21 | | 5 | 22 −, 21 , 24 |
| 22 | | 4 . 5 | 22 −, 21   22 − 19 |
| 23 | | 6 . 7 | 24 , 23 |
| 24 | | 6 | 21 , 24 , 23 |
| 25 | | 6 | 20 , 25 |
| 26 | | 4 . 5 | 11 $\overset{-}{,}$ 26 $\overset{-}{,}$ 29 |
| 27 | | 5 | 29 − 27   28 , 27 |
| 28 | | 5 | 29 , 28 , 27 |
| 29 | | 6 | 29 − 27   26 $\overset{-}{,}$ 29 , 28   29 , 31 |
| 30 | | 6 | 30 ; 11 |
| 31 | | 4 | 29 , 31 |

## Lustre of the stars in Perseus.

| | | | |
|---|---|---|---|
| 1 | | 6 | 4 , 1 − 3   4 − 1 |
| 2 | *g* | 6 | 3 , 2 |
| 3 | | 6 . 7 | 1 − 3 , 2 |
| 4 | | 6 | 4 , 1   4 − 1   4 − 9 |
| 5 | | 6 | 9 − 5 . 7   7 . 5 |
| 6 | *h* | 6 | 65 Androm , 6 , 63 Androm |
| 7 | *χ* | 7 . 6 | 5 . 7 . 8   8 ; 7 . 5 |
| 8 | | 7 | 7 . 8   9 −, 8 ; 7 |
| 9 | *i* | 6 | 4 − 9 −, 8   9 − 5 |
| 10 | | 7 | Does not exist |
| 11 | | 7 | 27 ; 11 − 13 |
| 12 | *q* | 6 | 28 . 12 . 14 |
| 13 | *θ* | 4 | 11 − 13 , 18   18 $\overset{-}{,}$ 13 |
| 14 | | 6 | 12 . 14   24 , 14 |

Lustre of the stars in Perseus.

| | | | |
|---|---|---|---|
| 15 | | 6 | Lost |
| 16 | $p^1$ | 4 | 16 , 22   16 -- 20 |
| 17 | *r* | 5.6 | 17 , 28 |
| 18 | τ | 5 | 13 , 18   18 -; 13 |
| 19 | | 6 | Does not exist |
| 20 | $p^2$ | 6 | 16 -- 20   28 - 20   43 , 20 |
| 21 | | 4.5 | 28 . 21 |
| 22 | π | 4 | 16 , 22 , 28 |
| 23 | γ | 3 | 23 , 25   23 -, 4 Trianguli |
| 24 | *s* | 6 | 28 . 24 , 14 |
| 25 | ϱ | 4 | 23 , 25 - 41   39 - 25 |
| 26 | β | 2.3 | 26 , 25   26 ; 25   26 --- 25<br>6 Arietis , 26 - 23 |
| 27 | ϰ | 5.4 | 27 ; 11 |
| 28 | ω | 5 | 22 , 28 . 12   17 , 28 . 24   28 . 21   28 - 20 |
| 29 | | 6 | 34 - 29 . 31 |
| 30 | | 6 | 32 - 30   36 - 30 |
| 31 | | 5.6 | 29 . 31 |
| 32 | ι | 6 | 32 - 30   32 - 36 |
| 33 | α | 2.3 | 33 -- 26   21 Androm , 33 . 43 Andromedæ |
| 34 | | 6 | 35 - 34   34 - 29 |
| 35 | σ | 5 | 37 , 35 - 34 |
| 36 | | 6.7 | 32 - 36 - 30 |
| 37 | ψ | 5 | 37 , 35 |
| 38 | $o^1$ | 6 | 46 , 38 , 59   41 - 38 - 46 |
| 39 | δ | 3 | 45 -- 39 - 25 |
| 40 | $o^2$ | 6 | 52 , 40 . 42   40 , 42 |
| 41 | ν | 4 | 25 - 41 , 46   41 - 38 |
| 42 | *n* | 6 | 40 . 42   40 , 42   54 ; 42 - 55 |
| 43 | A | 5 | 43 , 20 |
| 44 | ζ | 3 | 3 Aurigæ - 44   3 Aurigæ . 44   44 , 45 |
| 45 | ε | 3 | 44 , 45 - - 39 |

### Lustre of the stars in Perseus.

| | | | |
|---|---|---|---|
| 46 | *ξ* | 5 | 41 , 46 , 38   38 – 46   46 – 58 |
| 47 | λ | 4 | 51 . 47 , 53 |
| 48 | *c* | 5 | 48 , 51 |
| 49 | | 6.7 | 50 ; 49 |
| 50 | | 6.7 | 52 , 50 , 49 |
| 51 | *μ* | 4 | 48 , 51 . 47 |
| 52 | *f* | 5 | 52 , 50   53 . 52 , 40   58 – 52 |
| 53 | *d* | 6 | 47 , 53 . 52 |
| 54 | | 6 | 54 ; 42 |
| 55 | | 6 | 42 – 55 ; 56 |
| 56 | | 7 | 55 ; 56 |
| 57 | *m* | 6 | 59 . 57 |
| 58 | *e* | 5 | 46 – 58 – 52 |
| 59 | | 6 | 38 , 59 . 57 |

### Lustre of the stars in Sextans.

| | | | |
|---|---|---|---|
| 1 | | 5 | 1 – 2 |
| 2 | | 5 | 1 – 2 |
| 3 | | 6 | 8 –, 3 , 5   6 , 3 |
| 4 | | 6 | 7 , 4   4 – 12 |
| 5 | | 6 | 3 , 5 |
| 6 | | 6 | 8 – 6 , 3 |
| 7 | | 6 | 7 , 4 |
| 8 | | 6 | 8 –, 3   8 – 6 |
| 9 | | 6 | 12 , 9 . 13 |
| 10 | | 6 | 29 Leonis = –, 10 – 11 |
| 11 | | 5.6 | 10 – 11 |
| 12 | | 6 | 4 – 12 , 9 |
| 13 | | 6 | 9 . 13 |
| 14 | | 6 | 14 ; 19 |
| 15 | | 4 | 35 Hydræ , 15 – 32 Hydræ |

## Lustre of the stars in Sextans.

| | | | |
|---|---|---|---|
| 16 | | 6 | 19 , 16 |
| 17 | | 6 | 22 , 17 . 18 |
| 18 | | 6 | 17 . 18 – 20 |
| 19 | | 6 | 14 . 19 , 16 |
| 20 | | 6 | 18 – 20 , 21 |
| 21 | | 6 | 20 , 21 |
| 22 | | 6 | 22 , 17 |
| 23 | | 5 | 23 , 26 |
| 24 | | 6 | 28 . 24 –, 26     33 , 24 |
| 25 | | 6 | 25 . 26 |
| 26 | | 6 | 24 –, 26     25 . 26     23 , 26 - 31 |
| 27 | | 6 | 27 , 28 |
| 28 | | 5 | 29 – 28 . 24     27 , 28 – 32 |
| 29 | | 5 | 30 – 29 – 28 |
| 30 | | 5 | 32 Hydræ – 30 – 29 |
| 31 | | 6 | 26 – 31 |
| 32 | | 6 | 28 – 32 |
| 33 | | 6 | 33 , 24 |
| 34 | | 6 | 35 – 34 , 37 |
| 35 | | 6 | 35 – 34 |
| 36 | | 6 | 37 . 36 |
| 37 | | 6 | 55 Leonis –, 37 , 38     34 , 37 . 36 |
| 38 | | 6 | 37 , 38 |
| 39 | | 7 | 41 –, 39     40 , 39 |
| 40 | | 6 | 41 – 40 , 39 |
| 41 | | 6 | 41 –, 39     41 – 40 |

## Lustre of the stars in Taurus.

| | | | |
|---|---|---|---|
| 1 | *o* | 4 | 1 – 2 |
| 2 | ξ | 4 | 1 – 2 , 35 |
| 3 | | 6 | Does not exist |

Lustre of the stars in Taurus.

| | | | |
|---|---|---|---|
| 4 | *s* | 6 | 5 -, 4 -; 6    30 - 4 |
| 5 | *f* | 5 | 38 - 5 -, 4    5 - 30 |
| 6 | *t* | 6 | 4 -; 6, 12 |
| 7 | | 6 | 7 . 66 Arietis |
| 8 | | 6 | Does not exist |
| 9 | | 6 | Lost |
| 10 | | 4 . 5 | 38, 10, 49 |
| 11 | | 6 | 21 . 11, 22 |
| 12 | | 6 | 6, 12 |
| 13 | | 6 | 13, 14    13 - 14 |
| 14 | | 6 | 13, 14    13 - 14 |
| 15 | *n* | 6 | Does not exist |
| 16 | *g* | 7 | 18 . 16 . 21 |
| 17 | *b* | 5 | 27 . 17 . 20 |
| 18 | *m* | 7 | 28, 18 . 16 |
| 19 | *e* | 5 | 20, 19 . 23 |
| 20 | *c* | 6 | 17 . 20, 19 |
| 21 | *k* | 6 . 7 | 16 . 21 . 11 |
| 22 | *l* | 7 | 11, 22, 26 |
| 23 | *d* | 5 | 19 . 23 -, 28 |
| 24 | *p* | 7 | 26, 24 |
| 25 | η | 3 | 27 -, 27 |
| 26 | *s* | 7 . 8 | 22, 26, 24 |
| 27 | *f* | 6 | 25 -, 27 . 17 |
| 28 | *b* | 7 . 8 | 23 -, 28, 18 |
| 29 | $u^1$ | 6 | 29, 40 |
| 30 | *e* | 5 | 5 - 30 - 4    66, 30, 46 |
| 31 | $u^2$ | 6 | 40, 31 |
| 32 | | 6 | 53, 32 . 33 |
| 33 | | 7 | 32 . 33 |
| 34 | | 7 | 39 - 34 |
| 35 | λ | 4 | 2, 35 . 38    123 -; 35 |

Lustre of the stars in Taurus.

| | | | |
|---|---|---|---|
| 36 | | 7 | 43 ; 36 |
| 37 | A | 5 | 65 , 37 -- 39    37 -- 43 |
| 38 | $\nu$ | 4 | 35 . 38 - 5    38 , 10 |
| 39 | | 6 | 37 -- 39    51 - 39    43 - 39 - 34 |
| 40 | | 7 | 29 , 40 , 31    46 ; 40 , 45 |
| 41 | | 6 | 42 ; 41 ; 44 |
| 42 | $\psi$ | 5 | 52 . 42 ; 41 |
| 43 | $\omega^1$ | 6 | 37 -- 43 - 39    43 ; 36 |
| 44 | *p* | 6 | 41 ; 44 , 59 |
| 45 | | 7 | 40 , 45 |
| 46 | | 7 | 47 , 46 ; 40    30 , 46 . 93 |
| 47 | | 7 | 49 -, 47 , 46    47 . 60 |
| 48 | | 7 | 58 . 48 |
| 49 | $\mu$ | 4 | 10 , 49    49 -, 47    88 , 49 |
| 50 | $\omega^2$ | 6 | 65 - 50 - 56    50 , 67 |
| 51 | | 7 | 53 , 51 - 39 |
| 52 | $\varphi$ | 5 | 52 . 42 |
| 53 | | 7 | 56 , 53 , 51    53 , 32 |
| 54 | $\gamma$ | 3 | 77 -; 54 -- 58    54 -; 61    74 ; 54 |
| 55 | | 7 | 63 , 55 |
| 56 | | 7 | 50 - 56 , 53 |
| 57 | | 6 . 7 | 58 , 57 . 60 |
| 58 | *b* | 7 | 54 -- 58 . 48    58 , 57    73 - 58 -, 76    58 , 83 |
| 59 | $\chi$ | 5 | 44 , 59 |
| 60 | | 7 | 57 . 60    47 . 60 |
| 61 | $\delta^1$ | 4 | 54 -; 61 , 68 |
| 62 | | 7 | 72 - 62    65 ; 62 |
| 63 | | 6 | 64 -, 63 , 55 |
| 64 | $\delta^2$ | 4 | 68 - 64 -, 63 |
| 65 | $\kappa$ | 5 | 65 - 69    65 ; 62    65 - 50    65 , 37 |
| 66 | *r* | 5 | 66 , 30 |
| 67 | $\kappa^2$ | 5 | 69 - 67 . 72    50 , 67 ; 72 |

Lustre of the stars in Taurus.

| | | | |
|---|---|---|---|
| 68 | $\delta^3$ | 6 | 61 , 68 – 64 |
| 69 | $\upsilon^1$ | 5 | 65 – 69 – 67    94 . 69 |
| 70 | | 7 | 80 – 70 |
| 71 | | 7 | 71 ; 75 |
| 72 | $\upsilon^2$ | 6 | 72 – 62    67 . 72    67 ; 72    72 –, 95 |
| 73 | $\pi$ | 5 | 73 – 58    86 , 73 |
| 74 | $\varepsilon$ | 3 . 4 | 78 , 74 ; 54 |
| 75 | | 7 | 71 ; 75 , 81 |
| 76 | | 7 | 58 –, 76    83 – 76 |
| 77 | $\theta^1$ | 5 | 77 ‾, 54 |
| 78 | $\theta^2$ | 5 | 78 , 74 |
| 79 | *b* | 5 | 90 – – 79 , 83 |
| 80 | | 7 | 81 , 80 – 70    80 – 84    80 . 85 |
| 81 | | 7 | 75 , 81 , 80 |
| 82 | | 7 | Does not exist |
| 83 | | 7 | 79 , 83    58 , 83 – 76 |
| 84 | | 7 | 80 – 84 |
| 85 | | 7 | 80 . 85 |
| 86 | $\varrho$ | 5 | 86 , 73 |
| 87 | $\alpha$ | 1 | 58 Orionis – – 87    19 Orionis = ‾, 87<br>87 – – 78 Gemin |
| 88 | *d* | 5 | 90 , 88 , 49 |
| 89 | | 7 | 91 ; 89 |
| 90 | $c^1$ | 5 | 90 , 88    90 – – 79    90 – – 93 |
| 91 | $\sigma^1$ | 6 | 92 , 91 ; 89 |
| 92 | $\sigma^2$ | 6 | 92 , 91 |
| 93 | $c^2$ | 6 | 46 . 93    90 – – 93 |
| 94 | $\tau$ | 5 | 94 . 69 |
| 95 | | 6 . 7 | 72 –, 95 |
| 96 | | 6 | 4 Orionis – – 96    97 –, 96 |
| 97 | *i* | 6 | 4 Orionis , 97 –, 96 |
| 98 | *k* | 6 | 106 – 98 – 99    103 . 98 |

| Lustre of the stars in Taurus. | | | |
|---|---|---|---|
| 99 | | 6 | 98 – 99 , 107 |
| 100 | | 6 | Lost |
| 101 | | 6 | 107 ‾, 101 |
| 102 | *ι* | 4 | 102 = ‾, 106   102 , 104 |
| 103 | | 6 | 103 . 98 |
| 104 | *m* | 6 | 102 , 104 –, 106 |
| 105 | | 6 | 106 , 105 – 107 |
| 106 | $l^1$ | 6 | 102 = ‾, 106 , 105   106 . 109<br>104 –, 106 – – 107   106 – 98 |
| 107 | $l^2$ | 6 | 105 – 107   108 . 107   106 – – 107<br>99 , 107 ‾, 101 |
| 108 | | 7 | 109 –, 108   109 , 108 . 107 |
| 109 | *n* | 6 | 114 – 109 –, 108   106 . 109 , 108 |
| 110 | | 7 | 116 – 110 . 113   115 – 110 . 117   120 . 110 |
| 111 | | 6.7 | 111 , 116   111 , 115   119 – 111 |
| 112 | *β* | 2 | 112 –, 24 Orionis |
| 113 | | 6 | 110 . 113 |
| 114 | *ο* | 5 | 114 – 109 |
| 115 | | 7.8 | 111 , 115 – 110   122 . 115 |
| 116 | | 6.7 | 111 , 116 – 110 |
| 117 | | 7 | 110 . 117 |
| 118 | | 6 | 121 – 118 |
| 119 | | 7 | 119 – 111 |
| 120 | | 7 | 120 . 110 |
| 121 | | 6 | 121 – 118 |
| 122 | | 7 | 126 – 122 –, 129   122 , 130   122 . 115 |
| 123 | *ζ* | 3 | 123 ‾, 35   13 Gemin -, 123 , 7 Gemin |
| 124 | | 6.7 | Does not exist |
| 125 | | 3 | 132 , 125 |
| 126 | | 6 | 126 – 122 |
| 127 | | 6 | 130 – – 127 |
| 128 | | 6 | 129 , 128 |
| 129 | | 6 | 122 –, 129 , 128 |

## Lustre of the stars in Taurus.

| | | | |
|---|---|---|---|
| 130 | | 6 | 122 , 130 – – 127 |
| 131 | | 6 | 133 , 131 . 132     135 , 131 , 137 |
| 132 | | 4 | 139 , 132 , 125     131 . 132 . 135 |
| 133 | | 6 | 134 , 133 , 131     134 – 133 , 131 |
| 134 | | 6 | 134 , 133     134 – 133 |
| 135 | | 6 | 132 . 135 – 138     135 , 131 |
| 136 | | 5 | 136 ⁏ 139     44 Aurigæ , 136 |
| 137 | | 5 | 135 – 137     131 , 137 |
| 138 | | 6 | Does not exist |
| 139 | | 6 | 1 Gemin – 139 , 132     136 ⁏ 139 , 132 |
| 140 | | 6 | 141 . 140 |
| 141 | | 6 | 141 . 140 |

## Lustre of the stars in Triangulum.

| | | | |
|---|---|---|---|
| 1 | *d* | 6 | 3 . 1 |
| 2 | α | 4 | 4 –, 2 –, 9     31 Androm –, 2     2 – 99 Piscium |
| 3 | ε | 6 | 7 – 3 . 1 |
| 4 | β | 4 | 6 Arietis – 4     23 Persei –, 4 –, 2     4 =’ 31 Andromedæ |
| 5 | | 7 | 7 – 5 |
| 6 | ι | 6 | 6 , 7     6 , 10 |
| 7 | η | 6 | 8 – 7 – 3     6 , 7 – 5     7 ; 14 |
| 8 | δ | 5 | 9 –, 8 – 7 |
| 9 | γ | 4 | 2 –, 9 –, 8 |
| 10 | *a* | 6 | 6 , 10 ; 12 |
| 11 | *d* | 7 | 12 , 11 , 13 |
| 12 | *c* | 6 | 10 ; 12 ⁻, 13     12 , 11 |
| 13 | | 7 | 12 ⁻, 13     11 , 13 |
| 14 | | 6 | 7 ; 14 ; 15 |
| 15 | | 7 | 14 ; 15 |
| 16 | | 7 | 16 , 30 Arietis     33 Arietis –, 16 |

## *Notes to Auriga.*

23 Is 112 Tauri.

30 Is 32 Camelopardali.

45 "Oct. 5, 1798. The time of this star, in the observation "of Flamsteed, Vol. II. page 189, is marked : : but it cannot "be much out, as the star seems to be in the place assigned to "it by the British catalogue."

61. The RA in the *Atlas Cœlestis* requires a correction of — 42′.

## *Notes to Draco.*

10 Is 87 Ursæ.

12 and 13 Were never observed by Flamsteed, but are in La Caille's Catalogue of northern stars.

14 M. de la Lande says the star is not to be found. See Mr. Bode's *Ast. Jahr-Buch* for 1795, page 198.

I observed this star in a sweep of the heavens, June 2d, 1788. Its brightness was estimated Sept. 11, 1795; Sept. 24, 1796; Sept. 30, 1796; and Dec. 28, 1798; so that, if M. de la Lande is sure no cloud intervened when he looked for it, we may suspect it to be a changeable star.

15 The British catalogue requires + 30′ in RA.

35 The expression "$35 - \overline{40 + 41}$" in my estimation of brightness, means that, with the naked eye, 35 is a very little brighter than 40 and 41 together, taken as one star. For they are so near each other, that the eye alone cannot distinguish them from a single star. The British catalogue gives them 3′ farther asunder than they ought to be according to Flamsteed's observation, page 463. See also Mr. Bode's *Ast. Jahr-Buch* for 1785, page 173.

40 The estimation "40 — 41" was made with a 7-feet reflector, power 460.

56 Does not exist. FLAMSTEED has no observation of it. My double star II, 31, called 56 Draconis, is a star situated between 59 and 50, about 1½ degree from 59 towards 50.*

62 Does not exist. FLAMSTEED has no observation of it; but, if an error of two hours be supposed in the calculation of one of the observations of 31 Draconis, it will account for the insertion of this star.

72 Does not exist. There is an observation, page 173, which produced it; but, if we admit an error of 3′ in time in that observation, it will then belong to 71.

## *Notes to Lynx.*

7 Does not exist in the place pointed out by the British catalogue; but, in FLAMSTEED's observations, page 286, its time is marked : : and there is probably some considerable error.

20, 21, 22 The place of these stars in the heavens does not seem to agree with their situation in the Atlas.

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* When I say 1½ degree from 59 towards 50, it is to be understood that I express myself in degrees of a great circle. I have always used the same method of description in my catalogues of double stars; and, as these objects were pointed out for being viewed with telescopes of great magnifying power, which are generally not fixed, and therefore can give no right ascension, I am rather surprised to find that, in a catalogue published not many years ago, the author has taken my degrees of a great circle for degrees of right ascension. For instance, the double star IV, 82, where, in pointing out its place, I say, "above ¾ degree following the 16 Cephei, in a line parallel to β "and α Cassiopeæ," is placed in the zone from 15 to 19° of that author's catalogue, only 2′ 47″,5 of time following 16 Cephei, when it ought to have been at least 10′ or 11′ following.

I take this opportunity to mention that, in general, the same author's account of my double stars is extremely erroneous.

30 Is 58 Camelopardali.

35. FLAMSTEED has no observation of this star; but, as it is marked 7m in the British catalogue, and has a line allotted to it, my Atlas and stars have been numbered so as to take it in; and the numbers I have used with double stars and other objects where the stars in Lynx after the 35th are concerned, must be reckoned accordingly.

37 "Dec. 4, 1796, This star is nearer to 25 than it is marked "in the Atlas." The RA should be corrected + 1°.

### *Notes to Lyra.*

10 This is one of our periodical stars discovered by Mr. GOODRICKE; its period is about 6 days 9 hours. See Phil. Trans. Vol. LXXVI. page 197. The greatest variation of its light, as far as I have observed, is from "10 . 14 to "$\overline{6+7}\,\underset{-}{;}\,10$." The expression $\overline{6+7}$ is borrowed from algebra, and is always to be understood as has been explained in the note to 35 Draconis.

16 The British catalogue requires a correction of − 9° in PD; and this star will then agree with 12 Lyræ HEVELII.

19 The British catalogue requires a correction of + 8° in PD.

### *Notes to Perseus.*

5 FLAMSTEED has no observation of this star; but there is a star exactly in the place pointed out by the British catalogue.

10 Does not exist. FLAMSTEED never observed it.

12 "Sept. 5, 1798, This star, which has no time in FLAMSTEED's observations, is placed a little too forward; or requires "about + 10′ in RA."

14. " Sept. 4, 1798, The time of this star is marked doubtful " by FLAMSTEED, page 214; but it seems to be in the situation " where the British catalogue places it.'

15 Is lost. FLAMSTEED observed it Jan. 17, 1693, page 186; but it is not to be seen in the place pointed out by that observation. See BODE's *Ast. Jahr-Buch* for 1794, page 97.

19 Does not exist. There is an observation in page 185, which has produced this star, but it belongs to 18; for the star is lettered $\tau$, and a memorandum says, " Post transitum." See also BODE's *Ast. Jahr-Buch* for 1788, page 172.

24 " Sept. 4, 1798. The place of this star in the British " catalogue wants a correction of + 56′ in PD, and — 45′ " in RA."

26 Is a periodical star. It has been noticed in the last century as subject to change, by MONTANARI and MARALDI; but its being periodical was discovered by Mr. GOODRICKE, in 1783, who fixed the time of its change at 2 days 20 hours 48′ 56″. See Phil. Trans. Vol. LXXIV. page 287. I have seen it when brightest, " 6 Arietis, 26 – 23", and when most diminished, " 26, 25".

38. " Sept. 5, 1798, The British catalogue requires nearly " + 2° in RA, and — 13′ in PD; at least there is no other " star that can be taken for it."

42 " Sept. 4, 1798, The British catalogue requires a cor" rection of + 13 in PD."

## *Notes to Sextans.*

1 Is 10 Leonis.

10 Is 25 Leonis.

11 Is 28 Leonis.

12 " March 17, 1797, This star is misplaced in the British " catalogue; the PD should be corrected + 1°."

28 " March 21, 1797, This star is misplaced in the British " catalogue, and requires a correction of + 20′ in RA, and " + 1° in PD."

29 " March 21, 1797, The PD of this star in the British " catalogue requires + 1°."

## *Notes to Taurus.*

3 Does not exist. FLAMSTEED never observed it.

8 " Jan. 10, 1796, This star does not exist. FLAMSTEED " has no observation of it. There is a star about 9m not far " from the place."

9. " Dec. 28, 1798, This star is lost." M. DE LA LANDE says it is not to be found. See Mr. BODE's *Ast. Jahr-Buch* for 1795, page 198. FLAMSTEED has two complete observations of it, page 86, and page 506. We can hardly admit what Mr. BODE suggests, that this star, like the rest, has found its way into the British catalogue by some error of writing, or of calculating the observations; it will therefore be advisable to look for a future re-appearance of it, as it may prove to be a periodical or changeable one.

15 Does not exist. FLAMSTEED has no observation of it.

34 The estimation " 39 – 34" belongs to a star very nearly in the place where, according to FLAMSTEED's observation, 34 should be; but, as we know by calculation that the Georgian planet was about the situation where, the 13 of Dec. 1690, FLAMSTEED observed the supposed 34th, there can be no doubt but that he must have seen it, and taken it for a fixed star.

The magnitude, 6m, which he assigned to 34, agrees perfectly well with the lustre of the planet, compared with other stars which the same author has marked 6m; and, with his telescope, he could not have the most distant suspicion of its being any other object than a fixed star of about the 6th magnitude.

40 " March 4, 1796. The RA in the Atlas requires a cor-
" rection of about + 20′."

55 In the British catalogue, the PD requires — 8′.

56 The RA in the British catalogue requires — 15′.

82 Does not exist. FLAMSTEED did not observe this star, unless we admit a correction of the British catalogue — 1° 5′ in PD.

99 FLAMSTEED has no observation of this star; but, as there is one in the heavens, about a degree more north, the British catalogue requires probably a correction of — 1° in PD.

100 This star is lost. FLAMSTEED settled its place, page 369, and the observation seems to be a very good one.

103 FLAMSTEED has no observation of this star. How it came to be inserted in the British catalogue does not appear. I have given it as a double star V, 114, and here also estimated its brightness; but it must be remembered that my estimations do not strictly ascertain the place of objects. If, therefore, 103 does not exist, my double star, as well as the one here estimated, must be some star not far from the place assigned to 103 in the British catalogue.

112 Is 23 Aurigæ.

118 The Atlas should be corrected — 30′ in RA.

124 Does not exist; unless we admit a correction of $+ 1^{\circ} 4'$ in RA of the British catalogue.

138 Does not exist; but, as there is no time in FLAMSTEED's observation of this star, it is probably misplaced in the British catalogue, for there are several considerable stars in the neighbourhood.

### *Notes to Triangulum.*

1 "Nov. 2, 1798," This star, which has the time and zenith distance in FLAMSTEED's observations doubtful, seems to be nearly in the place where the British catalogue gives it. It should perhaps be a few minutes more north.

Slough, near Windsor,
Jan. 28, 1799.

# IX. *On a submarine Forest, on the east Coast of England. By* Joseph Correa de Serra, *LL.D. F.R.S. and A.S.*

Read February 28, 1799.

In geology, more perhaps than in any other branch of natural history, there exists a necessity of strictly separating the facts observed from the ideas which, in order to explain them, may occur to the mind of the observer. In the present state of this science, every well ascertained fact increases our still narrow stock of real knowledge; when, on the contrary, the reasonings we are enabled to make, are at best but ingenious guesses, which too often bias and mislead the judgment. I shall therefore endeavour, in this Paper, to give, first, a mere description of the object, unmixed with any systematical ideas, and shall afterwards offer such conjectures on its cause as seem to me to be fairly grounded on observation.

It was a common report in Lincolnshire, that a large extent of islets of moor, situated along its coast, and visible only in the lowest ebbs of the year, was chiefly composed of decayed trees. These islets are marked in Mitchell's chart of that coast, by the name of *clay huts;* and the village of Huttoft, opposite to which they principally lie, seems to have derived its name from them. In the month of September, 1796, I went to Sutton, on the coast of Lincolnshire, in company with the Right Hon. President of this Society, in order to examine their extent and nature. The 19th of the month, being the first day after the equinoctial full moon, when the lowest ebbs were to be expected, we went in a boat, at half past twelve at noon, and soon after set foot upon one of the largest islets then appearing. Its exposed surface was about thirty yards long, and twenty-five wide, when the tide was at the lowest. A great number of similar islets were visible round us, chiefly to the eastward and southward; and the fishermen, whose authority on this point is very competent, say, that similar moors are to be found along the whole coast, from Skegness to Grimsby, particularly off Addlethorpe and Mablethorpe. The channels dividing the islets were, at the time we saw them, wide, and of various depths; the islets themselves ranging generally from east to west in their largest dimension.

We visited them again in the ebbs of the 20th and 21st; and, though it generally did not ebb so far as we expected, we could notwithstanding ascertain, that they consisted almost entirely of roots, trunks, branches, and leaves of trees and shrubs, intermixed with some leaves of aquatic plants. The remains of some of these trees were still standing on their roots; while the trunks of the greater part lay scattered on the ground, in every possible direction. The bark of the trees and roots appeared generally as fresh as when they were growing; in that of the birches particularly, of which a great quantity was found, even the thin silvery membranes of the outer skin were discernible. The timber of all kinds, on the contrary, was decomposed and soft, in the greatest part of the trees; in some, however, it was firm, especially in the knots. The people of the country have often found among them very sound pieces of timber, fit to be employed for several œconomical purposes.

The sorts of wood which are still distinguishable are birch, fir, and oak. Other woods evidently exist in these islets, of some of which we found the leaves in the soil; but our present knowledge of the comparative anatomy of timbers, is not so far advanced as to afford us the means of pronouncing with confidence respecting their species. In general, the trunks, branches, and roots of the decayed trees, were considerably flattened; which is a phænomenon observed in the *Surtarbrand* or fossil wood of Iceland, and which SCHEUCHZER remarked also in the fossil wood found in the neighbourhood of the lake of Thun, in Switzerland.

The soil to which the trees are affixed, and in which they grew, is a soft greasy clay; but, for many inches above its surface, the soil is entirely composed of rotten leaves, scarcely distinguishable to the eye, many of which may be separated, by putting the soil in water, and dexterously and patiently using a spatula, or a blunt knife. By this method, I obtained some perfect leaves of *Ilex Aquifolium*, which are now in the Herbarium of the Right Hon. Sir JOSEPH BANKS; and some other leaves which, though less perfect, seem to belong to some species of willow. In this stratum of rotten leaves, we could also distinguish several roots of *Arundo Phragmites*.

These islets, according to the most accurate information, extend at least twelve miles in length, and about a mile in breadth, opposite to Sutton shore. The water without them, towards the sea, generally deepens suddenly, so as to form a steep bank. The channels between the several islets, when the islets are dry, in the lowest ebbs of the year, are from four to twelve feet deep; their bottoms are clay or sand, and their direction is generally from east to west.

A well dug at Sutton, by JOSHUA SEARBY, shows that a moor of the same nature is found under ground, in that part of the country, at the depth of sixteen feet; consequently, very nearly on the same level with that which constitutes the islets. The disposition of the strata was found to be as follows:

| | |
|---|---|
| Clay, | 16 feet. |
| Moor, similar to that of the islets, | from 3 to 4 ditto. |
| Soft moor, like the scowerings of a ditch bottom, mixed with shells and silt, | 20 ditto. |
| Marly clay, | 1 foot. |
| Chalk rock, | from 1 to 2 feet. |
| Clay, | 31 yards. |
| Gravel and water; the water has a chalybeate taste. | |

In order to ascertain the course of this subterraneous stratum of decayed vegetables, Sir JOSEPH BANKS directed a boring to be made, in the fields belonging to the Royal Society, in the parish of Mablethorpe. Moor, of a similar nature to that of SEARBY'S well, and of the islets, was found, very nearly on the same level, about four feet thick, and under it a soft clay.

The whole appearance of the rotten vegetables we observed, perfectly resembles, according to the remark of Sir JOSEPH BANKS, the moor which, in Blankeney fen, and in other parts of the East fen in Lincolnshire, is thrown up in the making of banks; barks, like those of the birch tree, being there also abundantly found. This moor extends over all the Lincolnshire fens, and has been traced as far as Peterborough, more than sixty miles to the south of Sutton. On the north side, the moory islets, according to the fishermen, extend as far as Grimsby, situated on the south side of the mouth of the Humber; and it is a remarkable circumstance, that in the large tracts of low lands which lie on the south banks of that river, a little above its mouth, there is a subterraneous stratum of decayed trees and shrubs, exactly like those we observed at Sutton; particularly at Axholme isle, a tract of ten miles in length, by five in breadth; and at Hatfield chase, which comprehends one hundred and eighty thousand acres. DUGDALE[*] had long ago made this observation, in the first of these places; and DE LA PRYME[†] in the second. The roots are there likewise standing in the places where they grew; the trunks lie prostrate. The woods are of the same species as at Sutton. Roots of aquatic plants and reeds are likewise mixed with them; and they are covered by a stratum of some yards of soil, the thickness of which, though not ascertained with exactness by the above-mentioned observers, we may easily conceive to correspond with that which covers the stratum of decayed wood at Sutton, by the circumstance of the roots being (according to Mr. RICHARDSON's observations[‡]) only visible when the water is low, where a channel was cut, which has left them uncovered.

Little doubt can be entertained of the moory islets of Sutton being a part of this extensive subterraneous stratum, which, by some inroad of the sea, has been there stripped of its covering of soil. The identity of the levels; that of the species of trees; the roots of these affixed, in both, to the soil where they grew; and, above all, the flattened shape of the trunks, branches, and roots, found in the islets, (which can only be accounted for by the heavy pressure of a superinduced stratum,) are sufficient reasons for this opinion.

Such a wide spread assemblage of vegetable ruins, lying almost in the same level, and that level generally under the common mark of low water, must naturally strike the observer, and give birth to the following questions.

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[*] History of Embanking and Draining. Chap. xxvii.

[†] Philos. Trans. Vol. XXII. p. 980.

[‡] Philos. Trans. Vol. XIX. p. 528.

1. What is the epoch of this destruction?
2. By what agency was it effected?

In answer to these questions, I will venture to submit the following reflections.

The fossil remains of vegetables hitherto dug up in so many parts of the globe, are, on a close inspection, found to belong to two very different states of our planet. The parts of vegetables, and their impressions, found in mountains of a cotaceous, schistous, or even sometimes of a calcareous nature, are chiefly of plants now existing between the tropics, which could neither have grown in the latitudes in which they are dug up, nor have been carried and deposited there by any of the acting forces under the present constitution of nature. The formation, indeed, of the very mountains in which they are buried, and the nature and disposition of the materials which compose them, are such as we cannot account for by any of the actions and re-actions which, in the actual state of things, take place on the surface of the earth. We must necessarily recur to that period in the history of our planet, when the surface of the ocean was at least so much above its present level, as to cover even the summits of these secondary mountains which contain the remains of tropical plants. The changes which these vegetables have suffered in their substance, is almost total; they commonly retain only the external configuration of what they originally were. Such is the state in which they have been found in England, by LLWYD; in France, by JUSSIEU; in the Netherlands, by BURTIN; not to mention instances in more distant countries. Some of the impressions or remains of plants found in soils of this nature, which were, by more ancient and less enlightened oryctologists, supposed to belong to plants actually growing in temperate and cold climates, seem, on accurate investigation, to have been parts of exotic vegetables. In fact, whether we suppose them to have grown near the spot where they are found, or to have been carried thither from different parts, by the force of an impelling flood, it is equally difficult to conceive, how organized beings, which, in order to live, require such a vast difference in temperature and in seasons, could live on the same spot, or how their remains could (from climates so widely distant) be brought together to the same place, by one common dislocating cause. To this ancient order of fossil vegetables belong whatever retains a vegetable shape, found in or near coal mines, and (to judge from the places where they have been found) the greater part of the agatized woods. But, from the species and present state of the trees which are the subject of this Memoir, and from the situation and nature of the soil in which they are found, it seems very clear that they do not belong to this primeval order of vegetable ruins.

The second order of fossil vegetables, comprehends those which are found in strata of clay or sand; materials which are the result of slow depositions of the sea or of rivers, agents still at work under the present constitution of our planet. These vegetable remains are found in such flat countries as may be considered to be of a new formation. Their vegetable organization still subsists, at least in part; and their vegetable substance has suffered a change only in colour, smell, or consistence; alterations which are produced by the development of their oily and bituminous parts, or by their natural progress towards rottenness. Such are the fossil vegetables found in Cornwall, by BORLASE; in Essex, by DERHAM; in Yorkshire, by DE LA PRYME, and RICHARDSON; and in foreign countries, by other naturalists. These vegetables are found at different depths, some of them much below the present level of the sea, but in clayey or sandy strata, (evidently belonging to modern formation,) and have, no doubt, been carried from their original place, and deposited there by the force of great rivers or currents, as it has been observed with respect to the Mississipi.* In many instances, however, these trees and shrubs are found standing on their roots, generally in low or marshy places, above, or very little below, the actual level of the sea.

To this last description of fossil vegetables, the decayed trees here described certainly belong. They have not been transported by currents or rivers; but, though standing in their native soil, we cannot suppose the level in which they are found, to be the same as that in which they grew. It would have been impossible for any of these trees and shrubs to vegetate so near the sea, and below the common level of its water: the waves would cover such tracts of land, and hinder any vegetation. We cannot conceive that the surface of the ocean has ever been lower than it now is; on the contrary, we are led by numberless phænomena to believe, that the level of the waters in our globe is much below what it was in former periods; we must therefore conclude, that the forest here described grew in a level high enough to permit its vegetation; and that the force (whatever it was) which destroyed it, lowered the level of the ground where it stood.

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* LA COUDRENIERE *sur les Depots du Mississipi. Journ. de Phys.* Vol. XXI, p. 230.

There is a force of subsidence (particularly in soft ground) which, being a natural consequence of gravity, slowly though perpetually operating, has its action sometimes quickened and rendered sudden by extraneous causes, for instance, by earthquakes. The slow effects of this force of subsidence have been accurately remarked in many places; examples also of its sudden action are recorded in almost every history of great earthquakes. The shores of Alexandria, according to DOLOMIEU's observations, are a foot lower than they were in the time of the PTOLEMIES. DONATI, in his natural history of the Adriatic, has remarked, seemingly with great accuracy, the effects of this subsidence at Venice; at Pola, in Istria; at Lissa, Bua, Zara, and Diclo, on the coast of Dalmatia. In England, BORLASE has given, in the Philosophical Transactions,* a curious observation of a subsidence, of at least sixteen feet, in the ground between Sampson and Trescaw islands, in Scilly. The soft and low ground between the towns of Thorne and Gowle, in Yorkshire, a space of many miles, has so much subsided in latter times, that some old men of Thorne affirmed, "that "whereas they could before see little of the steeple, (of Gowle,) "they now see the churchyard wall."† The instances of similar subsidence which might be mentioned, are innumerable.

This force of subsidence, suddenly acting by means of some earthquake, seems to me the most probable cause to which the actual submarine situation of the forest we are speaking of may be ascribed. It affords a simple easy explanation of the matter; its probability is supported by numberless instances of similar events; and it is not liable to the strong objections which exist against the hypothesis of the alternate depression and elevation of the level of the ocean; an opinion which, to be credible, requires the support of a great number of proofs, less equivocal than those which have hitherto been urged in its favour, even by the genius of a LAVOISIER.*

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* Vol. XLVIII. p. 62.

† GOUGH's edition of CAMDEN's Britannia, T. III. p. 35.

The stratum of soil, sixteen feet thick, placed above the decayed trees, seems to remove the epoch of their sinking and destruction, far beyond the reach of any historical knowledge. In CÆSAR's time, the level of the North sea appears to have been the same as in our days. He mentions the separation of the Wahal branch of the Rhine, and its junction to the Meuse; noticing the then existing distance from that junction to the sea; which agrees, according to D'ANVILLE's inquiries,† with the actual distance. Some of the Roman roads constructed by order of AUGUSTUS, under AGRIPPA's administration, leading to the maritime towns of Belgium, still exist, and reach the present shore.‡ The descriptions which Roman authors have left us, of the coasts, ports, and mouths of rivers, on both sides of the North sea, agree in general with their present state; except in the places ravaged by the inroads of this sea, more apt, from its form, to destroy the surrounding countries, than to increase them.

An exact resemblance exists between maritime Flanders and the opposite low coast of England, both in point of elevation above the sea, and of internal structure and arrangement of their soils. On both sides, strata of clay, silt, and sand, (often mixed with decayed vegetables,) are found near the surface; and, in both, these superior materials cover a very deep stratum of bluish or dark-coloured clay, unmixed with extraneous bodies. On both sides, they are the lowermost part of the soil, existing between the ridges of high lands,* on their respective sides of the same narrow sea. These two countries are certainly coeval; and, whatever proves that maritime Flanders has been for many ages out of the sea, must, in my opinion, prove also, that the forest we are speaking of was long before that time destroyed, and buried under a stratum of soil. Now it seems proved, from historical records, carefully collected by several learned members of the Brussels Academy, that no material change has happened to the lowermost part of maritime Flanders, during the period of the last two thousand years.†

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* *Mém. de l'Acad. de Paris.* 1789. p. 351.

† D'ANVILLE *Notice des Gaules.* p. 461.

‡ NICOL. BERGIER. *Hist. des grands Chemins des Romains. Ed. de Bruxelles.* Vol. II. p. 109.

I am therefore inclined to suppose the original catastrophe which buried this forest, to be of a very ancient date; but I suspect the inroad of the sea which uncovered the decayed trees of the islets of Sutton, to be comparatively recent. The state of the leaves and of the timber, and also the tradition of the neighbouring people, concur to strengthen this suspicion. Leaves and other delicate parts of plants, though they may be long preserved in a subterraneous situation, cannot remain uninjured, when exposed to the action of the waves and of the air. The people of the country believe, that their parish church once stood on the spot where the islets now are, and was submerged by the inroads of the sea; that, at very low water, their ancestors could even discern its ruins; that their present church was built to supply the place of that which the waves washed away; and that even their present clock belonged to the old church. So many concomitant though weak testimonies, incline me to believe their report, and to suppose that some of the stormy inundations of the North sea, which in these last centuries have washed away such large tracts of land on its shores, took away a soil resting on clay, and at last uncovered the trees which are the subject of this Paper.

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* These ridges of high lands, both on the British and Belgic side, must be very similar to each other, since they both contain parts of tropical plants in a fossil state. Cocoa nuts, and fruits of the areca, are found in the Belgic ridge. The petrified fruits of Sheppey, and other impressions of tropical plants, on this side of the water, are well known.

† Vide several papers in the Brussels *Mémoires*; also *Journ. de Phys.* T. XXXIV. p. 401.

# METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

# ROYAL SOCIETY,

BY ORDER OF THE

# PRESIDENT AND COUNCIL.

## METEOROLOGICAL JOURNAL

## for January, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| Jan. 1 | 38 | 8 | 0 | 39 | 54 | 29,67 | 86 | 0,240 | W | 1 | Fair. |
| | 45 | 2 | 0 | 45 | 55 | 29,67 | 81 | | W | 1 | Cloudy. |
| 2 | 39 | 8 | 0 | 40 | 54 | 29,77 | 84 | | W | 1 | Fair. |
| | 41 | 2 | 0 | 41 | 57 | 29,88 | 79 | | NW | 1 | Fine. |
| 3 | 34 | 8 | 0 | 36 | 54 | 29,94 | 83 | | NW | 1 | Cloudy. |
| | 40 | 2 | 0 | 39 | 54 | 29,97 | 83 | | E | 1 | Cloudy. |
| 4 | 36 | 8 | 0 | 42 | 53 | 29,95 | 87 | | SSE | 1 | Cloudy. |
| | 49 | 2 | 0 | 49 | 56 | 29,83 | 88 | | S | 2 | Cloudy. |
| 5 | 41 | 8 | 0 | 41 | 54 | 29,71 | 82 | 0,092 | SW | 2 | Cloudy. |
| | 44 | 2 | 0 | 44 | 57 | 29,68 | 78 | | W | 2 | Fine. |
| 6 | 37 | 8 | 0 | 41 | 53 | 29,46 | 84 | 0,022 | W | 2 | Cloudy. |
| | 43 | 2 | 0 | 42 | 55 | 29,83 | 78 | | NNE | 2 | Fair. |
| 7 | 31 | 8 | 0 | 31 | 53 | 30,27 | 82 | | NNE | 2 | Fine. |
| | 36 | 2 | 0 | 36 | 55 | 30,34 | 74 | | NE | 2 | Fine. |
| 8 | 30 | 8 | 0 | 31 | 52 | 30,46 | 74 | | NE | 2 | Cloudy. |
| | 33 | 2 | 0 | 32 | 53 | 30,48 | 73 | | NE | 2 | Cloudy. |
| 9 | 31 | 8 | 0 | 32 | 50 | 30,40 | 74 | | NE | 1 | Cloudy. |
| | 33 | 2 | 0 | 33 | 52 | 30,38 | 75 | | NE | 1 | Cloudy. |
| 10 | 30 | 8 | 0 | 30 | 49 | 30,33 | 78 | | NE | 1 | Cloudy. |
| | 32 | 2 | 0 | 32 | 51 | 30,30 | 76 | | NE | 1 | Cloudy. |
| 11 | 28 | 8 | 0 | 32 | 49 | 30,15 | 79 | | SE | 1 | Cloudy. |
| | 38 | 2 | 0 | 38 | 52 | 30,04 | 82 | | SE | 1 | Rain. |
| 12 | 33 | 8 | 0 | 33 | 50 | 29,89 | 87 | 0,115 | E | 1 | Snow. |
| | 36 | 2 | 0 | 36 | 51 | 29,92 | 87 | | E | 1 | Cloudy. |
| 13 | 33 | 8 | 0 | 33 | 50 | 30,11 | 85 | 0,033 | NE | 1 | Cloudy. |
| | 39 | 2 | 0 | 38 | 51 | 30,14 | 84 | | NW | 1 | Cloudy. |
| 14 | 32 | 8 | 0 | 36 | 50 | 30,11 | 87 | | SW | 1 | Cloudy. |
| | 47 | 2 | 0 | 47 | 51 | 29,98 | 80 | | W | 1 | Cloudy. |
| 15 | 44 | 8 | 0 | 46 | 51 | 29,86 | 89 | | W | 1 | Cloudy. |
| | 50 | 2 | 0 | 50 | 55 | 29,77 | 86 | | SW | 1 | Cloudy. |
| 16 | 43 | 8 | 0 | 43 | 53 | 29,55 | 85 | 0,038 | WSW | 1 | Cloudy. |
| | 44 | 2 | 0 | 44 | 54 | 29,49 | 84 | | SW | 1 | Cloudy. |

## METEOROLOGICAL JOURNAL
## for January, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| Jan. 17 | 33 | 8 | 0 | 33 | 50 | 29,39 | 88 | 0,053 | | | Foggy. |
| | 47 | 2 | 0 | 44 | 55 | 29,26 | 87 | | E | 1 | Cloudy. |
| 18 | 42 | 8 | 0 | 42 | 53 | 28,96 | 89 | 0,142 | SE | 1 | Fine. |
| | 48 | 2 | 0 | 47 | 56 | 29,02 | 82 | | SE | 2 | Cloudy. |
| 19 | 42 | 8 | 0 | 42 | 54 | 29,60 | 87 | | NE | 1 | Cloudy. |
| | 46 | 2 | 0 | 46 | 56 | 29,80 | 85 | | NE | 1 | Cloudy. |
| 20 | 37 | 8 | 0 | 42 | 53 | 30,22 | 88 | | SW | 1 | Cloudy. |
| | 49 | 2 | 0 | 49 | 55 | 30,31 | 89 | | SSW | 1 | Cloudy. |
| 21 | 47 | 8 | 0 | 48 | 55 | 30,42 | 90 | | S | 1 | Cloudy. |
| | 53 | 2 | 0 | 53 | 57 | 30,43 | 88 | | S | 1 | Cloudy. |
| 22 | 48 | 8 | 0 | 48 | 55 | 30,15 | 87 | | SSE | 2 | Cloudy. |
| | 51 | 2 | 0 | 51 | 56 | 30,05 | 88 | | SW | 2 | Cloudy. |
| 23 | 35 | 8 | 0 | 35 | 54 | 30,48 | 82 | 0,020 | WSW | 1 | Fair. |
| | 43 | 2 | 0 | 43 | 59 | 30,52 | 77 | | NW | 1 | Fine. |
| 24 | 35 | 8 | 0 | 40 | 53 | 30,28 | 82 | | W | 2 | Cloudy. |
| | 48 | 2 | 0 | 46 | 57 | 30,10 | 83 | | SW | 2 | Cloudy. |
| 25 | 37 | 8 | 0 | 38 | 54 | 30,21 | 80 | | NW | 2 | Fair. |
| | 42 | 2 | 0 | 41 | 56 | 30,33 | 75 | | NE | 2 | Cloudy. |
| 26 | 29 | 8 | 0 | 29 | 53 | 30,41 | 80 | | WSW | 1 | Fair. |
| | 41 | 2 | 0 | 41 | 55 | 30,30 | 78 | | SSW | 1 | Cloudy. |
| 27 | 38 | 8 | 0 | 42 | 53 | 30,02 | 88 | 0,025 | SSW | 1 | Rain. |
| | 47 | 2 | 0 | 47 | 56 | 30,05 | 82 | | N | 1 | Cloudy. |
| 28 | 30 | 8 | 0 | 30 | 53 | 30,30 | 84 | | SW | 1 | Fine. |
| | 40 | 2 | 0 | 40 | 56 | 30,24 | 79 | | S | 2 | Fine. |
| 29 | 34 | 8 | 0 | 38 | 52 | 29,65 | 81 | | S | 2 | Cloudy. |
| | 43 | 2 | 0 | 43 | 54 | 29,40 | 83 | | SSE | 2 | Cloudy. |
| 30 | 32 | 8 | 0 | 32 | 51 | 29,49 | 87 | 0,150 | SW | 1 | Fine. |
| | 46 | 2 | 0 | 43 | 55 | 29,44 | 82 | | S | 2 | Cloudy. |
| 31 | 40 | 8 | 0 | 40 | 51 | 29,10 | 87 | 0,175 | S | 2 | Fine. [Much wind last night. |
| | 44 | 2 | 0 | 41 | 54 | 29,16 | 84 | | WSW | 2 | Rain. |

## METEOROLOGICAL JOURNAL

## for February, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| Feb. 1 | 33 | 7 | 0 | 33 | 48 | 29,80 | 83 | 0,063 | WSW | 2 | Fine. |
| | 45 | 2 | 0 | 44 | 55 | 29,91 | 79 | | W | 2 | Fine. |
| 2 | 39 | 7 | 0 | 41 | 53 | 29,98 | 88 | 0,224 | SE | 1 | Rain. |
| | 53 | 2 | 0 | 53 | 56 | 29,93 | 89 | | WSW | 1 | Cloudy. |
| 3 | 47 | 7 | 0 | 47 | 53 | 29,99 | 89 | | SW | 2 | Cloudy. |
| | 54 | 2 | 0 | 54 | 57 | 30,01 | 86 | | WSW | 2 | Cloudy. |
| 4 | 39 | 7 | 0 | 39 | 55 | 30,02 | 82 | | WSW | 1 | Fair. |
| | 45 | 2 | 0 | 45 | 58 | 30,33 | 81 | | NE | 1 | Fair. |
| 5 | 34 | 7 | 0 | 35 | 55 | 30,59 | 83 | | ENE | 1 | Cloudy. |
| | 42 | 2 | 0 | 42 | 56 | 30,60 | 77 | | E | 1 | Cloudy. |
| 6 | 31 | 7 | 0 | 31 | 53 | 30,58 | 86 | | E | 1 | Cloudy. |
| | 39 | 2 | 0 | 39 | 56 | 30,60 | 82 | | NE | 1 | Foggy. |
| 7 | 31 | 7 | 0 | 31 | 53 | 30,73 | 85 | | NE | 1 | Foggy. |
| | 40 | 2 | 0 | 40 | 55 | 30,76 | 82 | | SE | 1 | Foggy. |
| 8 | 28 | 7 | 0 | 29 | 53 | 30,68 | 83 | | SW | 1 | Fair. |
| | 41 | 2 | 0 | 41 | 56 | 30,66 | 75 | | W | 1 | Fair. |
| 9 | 32 | 7 | 0 | 33 | 54 | 30,51 | 84 | | SW | 1 | Cloudy. |
| | 42 | 2 | 0 | 42 | 55 | 30,45 | 81 | | WNW | 1 | Fair. |
| 10 | 41 | 7 | 0 | 43 | 55 | 30,39 | 87 | | SW | 1 | Cloudy. |
| | 48 | 2 | 0 | 48 | 57 | 30,37 | 82 | | WNW | 1 | Cloudy. |
| 11 | 38 | 7 | 0 | 38 | 55 | 30,38 | 87 | | SW | 1 | Fine. |
| | 50 | 2 | 0 | 50 | 57 | 30,42 | 86 | | WNW | 1 | Cloudy. |
| 12 | 38 | 7 | 0 | 40 | 55 | 30,47 | 85 | | SW | 1 | Cloudy. |
| | 46 | 2 | 0 | 46 | 57 | 30,46 | 79 | | WSW | 1 | Cloudy. |
| 13 | 42 | 7 | 0 | 42 | 54 | 30,40 | 81 | | SW | 1 | Cloudy. |
| | 50 | 2 | 0 | 50 | 57 | 30,38 | 77 | | WSW | 1 | Cloudy. |
| 14 | 41 | 7 | 0 | 43 | 54 | 30,34 | 86 | | SW | 1 | Cloudy. |
| | 50 | 2 | 0 | 50 | 57 | 30,23 | 87 | | SW | 2 | Cloudy. |
| 15 | 50 | 7 | 0 | 50 | 57 | 29,96 | 88 | | SSW | 2 | Cloudy. |
| | 54 | 2 | 0 | 54 | 58 | 29,87 | 89 | | SSW | 2 | Rain. |
| 16 | 34 | 7 | 0 | 34 | 56 | 29,98 | 81 | 0,120 | WNW | 2 | Fine. |
| | 43 | 2 | 0 | 43 | 57 | 29,96 | 74 | | NW | 2 | Fine. |

## METEOROLOGICAL JOURNAL

## for February, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| Feb. 17 | 31 | 7 | 0 | 32 | 55 | 29,95 | 82 | | NE | 1 | Cloudy. |
| | 37 | 2 | 0 | 37 | 54 | 29,95 | 76 | | NE | 1 | Cloudy. |
| 18 | 26 | 7 | 0 | 26 | 52 | 30,12 | 82 | | NE | 1 | Fair. |
| | 38 | 2 | 0 | 37 | 55 | 30,10 | 78 | | NE | 2 | Fair. |
| 19 | 24 | 7 | 0 | 24 | 50 | 29,94 | 80 | | NE | 1 | Fine. |
| | 34 | 2 | 0 | 34 | 53 | 29,85 | 79 | | NW | 2 | Fair. |
| 20 | 25 | 7 | 0 | 25 | 49 | 29,60 | 80 | | E | 1 | Fine. |
| | 38 | 2 | 0 | 38 | 53 | 29,59 | 75 | | SSE | 1 | Fair. |
| 21 | 33 | 7 | 0 | 33 | 50 | 29,70 | 86 | | W | 1 | Cloudy. |
| | 43 | 2 | 0 | 43 | 53 | 29,69 | 75 | | S | 1 | Fair. |
| 22 | 35 | 7 | 0 | 35 | 50 | 29,23 | 87 | 0,058 | SSW | 2 | Fine. |
| | 45 | 2 | 0 | 45 | 54 | 29,38 | 71 | | W | 2 | Fine. |
| 23 | 33 | 7 | 0 | 33 | 51 | 29,77 | 83 | | SW | 1 | Cloudy. |
| | 47 | 2 | 0 | 45 | 53 | 29,72 | 84 | | S | 1 | Rain. |
| 24 | 45 | 7 | 0 | 48 | 53 | 29,54 | 90 | 0,080 | S | 2 | Rain. |
| | 49 | 2 | 0 | 48 | 55 | 29,60 | 90 | | NW | 2 | Cloudy. |
| 25 | 36 | 7 | 0 | 36 | 53 | 29,64 | 84 | 0,148 | SW | 1 | Fine. |
| | 46 | 2 | 0 | 46 | 56 | 29,70 | 77 | | SW | 2 | Fine. |
| 26 | 34 | 7 | 0 | 34 | 53 | 29,86 | 86 | | SW | 2 | Fine. |
| | 46 | 2 | 0 | 45 | 55 | 30,02 | 77 | | NW | 1 | Fair. |
| 27 | 31 | 7 | 0 | 31 | 53 | 30,28 | 83 | | SW | 1 | Fair. |
| | 43 | 2 | 0 | 43 | 57 | 30,32 | 74 | | NNW | 1 | Fair. |
| 28 | 32 | 7 | 0 | 32 | 53 | 30,34 | 84 | | E | 1 | Fair. |
| | 46 | 2 | 0 | 46 | 56 | 30,27 | 76 | | ESE | 1 | Fine. |

## METEOROLOGICAL JOURNAL

## for March, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| Mar. 1 | 31 | 7 | 0 | 31 | 53 | 30,20 | 83 | | NE | 1 | Fair. |
| | 50 | 2 | 0 | 49 | 56 | 30,20 | 77 | | SSE | 1 | Fair. |
| 2 | 38 | 7 | 0 | 38 | 54 | 30,21 | 85 | | E | 1 | Hazy. |
| | 54 | 2 | 0 | 54 | 58 | 30,24 | 78 | | SSE | 1 | Fair. |
| 3 | 46 | 7 | 0 | 46 | 56 | 30,34 | 90 | | SSE | 1 | Cloudy. |
| | 58 | 2 | 0 | 57 | 58 | 30,34 | 77 | | SE | 1 | Fine. |
| 4 | 46 | 7 | 0 | 46 | 57 | 30,37 | 88 | | SW | 1 | Cloudy. |
| | 55 | 2 | 0 | 54 | 59 | 30,37 | 85 | | SW | 1 | Cloudy. |
| 5 | 49 | 7 | 0 | 49 | 58 | 30,27 | 89 | | SW | 1 | Cloudy. |
| | 56 | 2 | 0 | 56 | 60 | 30,27 | 82 | | SW | 1 | Cloudy. |
| 6 | 49 | 7 | 0 | 49 | 58 | 30,15 | 86 | | SW | 1 | Cloudy. |
| | 56 | 2 | 0 | 56 | 60 | 30,16 | 83 | | NW | 1 | Cloudy. |
| 7 | 47 | 7 | 0 | 47 | 59 | 30,18 | 76 | | E | 1 | Cloudy. |
| | 54 | 2 | 0 | 54 | 59 | 30,18 | 76 | | ESE | 1 | Cloudy. |
| 8 | 48 | 7 | 0 | 48 | 58 | 30,13 | 87 | 0,038 | SSW | 1 | Cloudy. |
| | 55 | 2 | 0 | 55 | 59 | 30,12 | 79 | | NW | 1 | Cloudy. |
| 9 | 42 | 7 | 0 | 42 | 58 | 30,11 | 83 | | E | 1 | Cloudy. |
| | 58 | 2 | 0 | 58 | 61 | 30,05 | 76 | | SSE | 1 | Cloudy. |
| 10 | 44 | 7 | 0 | 44 | 59 | 29,88 | 84 | | SW | 1 | Cloudy. |
| | 57 | 2 | 0 | 56 | 60 | 29,79 | 72 | | W | 1 | Cloudy. |
| 11 | 34 | 7 | 0 | 34 | 58 | 29,82 | 82 | 0,150 | ENE | 1 | Fair. |
| | 41 | 2 | 0 | 41 | 60 | 29,88 | 72 | | N | 1 | Fine. |
| 12 | 30 | 7 | 0 | 30 | 55 | 30,05 | 80 | | NE | 1 | Fine. |
| | 42 | 2 | 0 | 42 | 57 | 30,04 | 72 | | E | 1 | Fair. |
| 13 | 31 | 7 | 0 | 31 | 54 | 29,93 | 78 | | E | 1 | Fair. |
| | 41 | 2 | 0 | 41 | 55 | 29,88 | 70 | | E | 2 | Fine. |
| 14 | 31 | 7 | 0 | 31 | 54 | 30,06 | 80 | | NE | 1 | Fair. |
| | 45 | 2 | 0 | 45 | 57 | 30,12 | 74 | | NE | 1 | Fair. |
| 15 | 33 | 7 | 0 | 33 | 53 | 30,16 | 81 | | WSW | 1 | Fair. |
| | 55 | 2 | 0 | 55 | 55 | 30,10 | 76 | | SW | 1 | Cloudy. |
| 16 | 44 | 7 | 0 | 44 | 56 | 29,88 | 85 | | WSW | 1 | Cloudy. |
| | 52 | 2 | 0 | 50 | 57 | 29,70 | 74 | | WNW | 1 | Fine. |

## METEOROLOGICAL JOURNAL

## for March, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. |  | Therm. without. | Therm. within. | Barom. | Hy-gro-me-ter. | Rain. | Winds. |  | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
|  | ° | H. | M. | ° | ° | Inches. |  | Inches. | Points. | Str. |  |
| Mar. 17 | 36 | 7 | 0 | 38 | 54 | 29,40 | 82 |  | WNW | 1 | Cloudy. |
|  | 48 | 2 | 0 | 48 | 56 | 29,27 | 73 |  | WNW | 2 | Cloudy. |
| 18 | 35 | 7 | 0 | 36 | 55 | 29,18 | 83 | 0,025 | N | 1 | Cloudy. |
|  | 44 | 2 | 0 | 41 | 57 | 29,25 | 81 |  | NE | 1 | Rain. |
| 19 | 36 | 7 | 0 | 36 | 54 | 29,43 | 85 | 0,037 | NE | 1 | Cloudy. |
|  | 43 | 2 | 0 | 42 | 56 | 29,49 | 82 |  | NE | 1 | Cloudy. |
| 20 | 30 | 7 | 0 | 30 | 53 | 29,55 | 84 | 0,058 | NE | 1 | Cloudy. |
|  | 42 | 2 | 0 | 42 | 54 | 29,54 | 74 |  | NE | 1 | Hazy. |
| 21 | 30 | 7 | 0 | 31 | 52 | 29,70 | 84 |  | NE | 1 | Fine. |
|  | 44 | 2 | 0 | 42 | 54 | 29,83 | 80 |  | NE | 1 | Cloudy. |
| 22 | 30 | 7 | 0 | 31 | 50 | 29,88 | 86 |  | NE | 1 | Cloudy. |
|  | 43 | 2 | 0 | 43 | 54 | 29,90 | 76 |  | NE | 2 | Cloudy. |
| 23 | 31 | 7 | 0 | 33 | 51 | 30,08 | 81 |  | NE | 1 | Cloudy. |
|  | 44 | 2 | 0 | 44 | 53 | 30,15 | 73 |  | NE | 1 | Cloudy. |
| 24 | 36 | 7 | 0 | 36 | 51 | 30,20 | 83 |  | NE | 1 | Cloudy. |
|  | 43 | 2 | 0 | 43 | 53 | 30,13 | 75 |  | NE | 1 | Cloudy. |
| 25 | 39 | 7 | 0 | 40 | 53 | 30,00 | 84 |  | E | 1 | Cloudy. |
|  | 48 | 2 | 0 | 48 | 54 | 29,97 | 77 |  | E | 1 | Cloudy. |
| 26 | 36 | 7 | 0 | 38 | 52 | 29,89 | 85 |  | NE | 1 | Cloudy. |
|  | 49 | 2 | 0 | 47 | 54 | 29,83 | 75 |  | NE | 1 | Fair. |
| 27 | 35 | 7 | 0 | 36 | 52 | 29,94 | 83 |  | NE | 1 | Cloudy. |
|  | 46 | 2 | 0 | 46 | 54 | 29,98 | 75 |  | NE | 1 | Cloudy. |
| 28 | 39 | 7 | 0 | 39 | 51 | 29,90 | 78 |  | NW | 1 | Cloudy. |
|  | 48 | 2 | 0 | 48 | 54 | 29,80 | 81 |  | N | 1 | Cloudy. |
| 29 | 40 | 7 | 0 | 40 | 53 | 29,72 | 85 |  | NE | 1 | Cloudy. |
|  | 47 | 2 | 0 | 46 | 55 | 29,72 | 81 |  | NE | 1 | Cloudy. |
| 30 | 36 | 7 | 0 | 36 | 53 | 29,58 | 81 | 0,025 | E | 1 | Rain. |
|  | 44 | 2 | 0 | 44 | 56 | 29,61 | 76 |  | E | 1 | Cloudy. |
| 31 | 30 | 7 | 0 | 30 | 52 | 29,87 | 77 |  | NE | 1 | Fair. |
|  | 40 | 2 | 0 | 40 | 56 | 29,91 | 68 |  | NW | 2 | Fair. |

## METEOROLOGICAL JOURNAL

## for April, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| April 1 | 30 | 7 | 0 | 31 | 53 | 30,00 | 77 | | W | 1 | Fair. |
| | 46 | 2 | 0 | 45 | 56 | 30,00 | 70 | | NW | 2 | Fair. |
| 2 | 36 | 7 | 0 | 40 | 53 | 29,87 | 79 | | SE | 2 | Cloudy. |
| | 49 | 2 | 0 | 48 | 55 | 29,75 | 71 | | SE | 2 | Cloudy. |
| 3 | 39 | 7 | 0 | 40 | 53 | 29,46 | 84 | | W | 2 | Fine. |
| | 54 | 2 | 0 | 54 | 57 | 29,54 | 71 | | W | 1 | Cloudy. |
| 4 | 47 | 7 | 0 | 51 | 56 | 29,27 | 89 | 0,335 | S | 2 | Rain. |
| | 55 | 2 | 0 | 54 | 57 | 29,30 | 83 | | S | 2 | Cloudy. |
| 5 | 48 | 7 | 0 | 49 | 55 | 29,47 | 82 | 0,018 | S | 2 | Cloudy. |
| | 58 | 2 | 0 | 57 | 59 | 29,57 | 73 | | S | 2 | Fair. |
| 6 | 48 | 7 | 0 | 48 | 55 | 29,65 | 78 | 0,023 | S | 2 | Fair. |
| | 58 | 2 | 0 | 56 | 60 | 29,86 | 75 | | S | 2 | Cloudy. |
| 7 | 47 | 7 | 0 | 50 | 58 | 30,08 | 84 | 0,038 | S | 1 | Fair. |
| | 62 | 2 | 0 | 62 | 62 | 30,11 | 73 | | S | 2 | Fair. |
| 8 | 46 | 7 | 0 | 50 | 60 | 30,26 | 83 | | S | 1 | Fine. |
| | 66 | 2 | 0 | 64 | 62 | 30,31 | 72 | | S | 1 | Fair. |
| 9 | 43 | 7 | 0 | 45 | 60 | 30,38 | 82 | | NE | 1 | Hazy. |
| | 66 | 2 | 0 | 66 | 63 | 30,38 | 70 | | E | 1 | Fine. |
| 10 | 46 | 7 | 0 | 48 | 62 | 30,33 | 83 | | E | 1 | Fine. |
| | 67 | 2 | 0 | 67 | 64 | 30,29 | 70 | | E | 2 | Fine. |
| 11 | 49 | 7 | 0 | 51 | 61 | 30,33 | 71 | | E | 2 | Fine. |
| | 61 | 2 | 0 | 61 | 66 | 30,33 | 66 | | E | 2 | Fine. |
| 12 | 45 | 7 | 0 | 48 | 60 | 30,26 | 68 | | E | 2 | Fine. |
| | 58 | 2 | 0 | 58 | 63 | 30,19 | 66 | | E | 2 | Fine. |
| 13 | 43 | 7 | 0 | 45 | 60 | 30,04 | 73 | | E | 1 | Fair. |
| | 63 | 2 | 0 | 62 | 63 | 29,99 | 66 | | E | 2 | Fine. |
| 14 | 47 | 7 | 0 | 50 | 61 | 29,95 | 76 | | E | 2 | Fine. |
| | 60 | 2 | 0 | 58 | 63 | 29,89 | 75 | | SE | 2 | Cloudy. |
| 15 | 49 | 7 | 0 | 51 | 61 | 29,88 | 80 | | SSW | 2 | Cloudy. |
| | 60 | 2 | 0 | 60 | 62 | 29,88 | 72 | | S | 2 | Cloudy. |
| 16 | 50 | 7 | 0 | 50 | 60 | 29,60 | 78 | 0,020 | SSW | 2 | Cloudy. |
| | 61 | 2 | 0 | 61 | 63 | 29.68 | 69 | | SSW | 2 | Fair. |

## METEOROLOGICAL JOURNAL

### for April, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. |  | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. |  | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
|  |  | H. | M. | ° | ° | Inches. |  | Inches. | Points. | Str. |  |
|  | ° |  |  |  |  |  |  |  |  |  |  |
| Apr. 17 | 47 | 7 | 0 | 49 | 61 | 29,72 | 77 |  | ENE | 1 | Cloudy. |
|  | 59 | 2 | 0 | 59 | 62 | 29,68 | 74 |  | E | 1 | Cloudy. |
| 18 | 47 | 7 | 0 | 48 | 61 | 29,72 | 81 |  | NE | 1 | Cloudy. |
|  | 56 | 2 | 0 | 56 | 62 | 29,75 | 74 |  | NE | 1 | Cloudy. |
| 19 | 45 | 7 | 0 | 46 | 60 | 29,70 | 80 | 0,083 | E | 1 | Cloudy. |
|  | 56 | 2 | 0 | 54 | 61 | 29,60 | 76 |  | SE | 1 | Cloudy. |
| 20 | 43 | 7 | 0 | 45 | 60 | 29,88 | * |  | NE | 2 | Fair. |
|  | 50 | 2 | 0 | 50 | 61 | 30,02 |  |  | NE | 2 | Fair. |
| 21 | 40 | 7 | 0 | 42 | 58 | 30,21 |  |  | NE | 1 | Cloudy. |
|  | 52 | 2 | 0 | 52 | 58 | 30,21 |  |  | NE | 1 | Cloudy. |
| 22 | 38 | 7 | 0 | 41 | 57 | 30,23 |  |  | ESE | 1 | Fine. |
|  | 55 | 2 | 0 | 55 | 60 | 30,18 |  |  | E | 1 | Fine. |
| 23 | 38 | 7 | 0 | 43 | 58 | 30,15 |  |  | E | 1 | Fair. |
|  | 60 | 2 | 0 | 60 | 60 | 30,08 |  |  | E | 1 | Fine. |
| 24 | 43 | 7 | 0 | 48 | 58 | 30,12 |  |  | NE | 1 | Hazy. |
|  | 65 | 2 | 0 | 65 | 59 | 30,14 |  |  | E | 1 | Fine. |
| 25 | 43 | 7 | 0 | 47 | 59 | 30,20 |  |  | ENE | 1 | Fine. |
|  | 59 | 2 | 0 | 59 | 60 | 30,20 |  |  | ESE | 1 | Fine. |
| 26 | 44 | 7 | 0 | 47 | 59 | 30,20 |  |  | E | 1 | Fair. |
|  | 67 | 2 | 0 | 67 | 60 | 30,11 |  |  | SSE | 1 | Fine. |
| 27 | 48 | 7 | 0 | 52 | 60 | 30,08 |  |  | SSW | 1 | Hazy. |
|  | 69 | 2 | 0 | 69 | 62 | 30,03 |  |  | SSE | 1 | Fine. |
| 28 | 47 | 7 | 0 | 52 | 61 | 29,98 |  |  | NE | 1 | Fair. |
|  | 66 | 2 | 0 | 65 | 64 | 29,94 |  |  | E | 1 | Fine. |
| 29 | 45 | 7 | 0 | 48 | 62 | 29,89 |  |  | E | 1 | Cloudy. |
|  | 57 | 2 | 0 | 57 | 61 | 29,87 |  |  | NE | 1 | Cloudy. |
| 30 | 42 | 7 | 0 | 49 | 60 | 29,84 |  |  | NE | 1 | Cloudy. |
|  | 58 | 2 | 0 | 57 | 60 | 29,81 |  |  | E | 1 | Fair. |

* The whalebone of the Hygrometer slipped out of its place.

## METEOROLOGICAL JOURNAL
## for May, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| May 1 | 45 | 7 | 0 | 46 | 59 | 29,85 | 52 | | NE | 1 | Cloudy. |
| | 54 | 2 | 0 | 53 | 58 | 29,85 | 48 | | NE | 1 | Cloudy. |
| 2 | 48 | 7 | 0 | 49 | 58 | 30,02 | 56 | | NE | 1 | Cloudy. |
| | 58 | 2 | 0 | 57 | 58 | 30,08 | 45 | | NE | 1 | Cloudy. |
| 3 | 43 | 7 | 0 | 48 | 58 | 30,23 | 53 | | NE | 1 | Fair. |
| | 64 | 2 | 0 | 64 | 60 | 30,24 | 39 | | NE | 1 | Fine. |
| 4 | 46 | 7 | 0 | 46 | 59 | 30,28 | 54 | | NE | 1 | Cloudy. |
| | 59 | 2 | 0 | 59 | 59 | 30,21 | 45 | | NE | 1 | Fair. |
| 5 | 47 | 7 | 0 | 50 | 59 | 30,10 | 54 | | E | 1 | Cloudy. |
| | 58 | 2 | 0 | 58 | 59 | 30,10 | 48 | | SSE | 1 | Cloudy. |
| 6 | 49 | 7 | 0 | 51 | 59 | 30,08 | 60 | | SW | 1 | Cloudy. |
| | 63 | 2 | 0 | 60 | 60 | 30,09 | 44 | | N | 1 | Cloudy. |
| 7 | 50 | 7 | 0 | 50 | 59 | 30,12 | 66 | 0,375 | S | 1 | Rain. |
| | 63 | 2 | 0 | 62 | 60 | 30,12 | 50 | | S | 1 | Cloudy. |
| 8 | 46 | 7 | 0 | 47 | 59 | 30,13 | 69 | | SSW | 1 | Cloudy. |
| | 66 | 2 | 0 | 66 | 61 | 30,09 | 45 | | S | 1 | Fine. |
| 9 | 48 | 7 | 0 | 50 | 60 | 30,03 | 57 | 0,100 | SW | 1 | Fine. |
| | 67 | 2 | 0 | 66 | 60 | 29,97 | 50 | | SSW | 1 | Cloudy. |
| 10 | 47 | 7 | 0 | 49 | 60 | 29,87 | 60 | | SSW | 1 | Fine. |
| | 67 | 2 | 0 | 67 | 61 | 29,82 | 42 | | SW | 1 | Fair. |
| 11 | 47 | 7 | 0 | 49 | 60 | 29,70 | 55 | | NW | 1 | Fine. |
| | 63 | 2 | 0 | 57 | 59 | 29,68 | 56 | | SSW | 1 | Rain. |
| 12 | 49 | 7 | 0 | 51 | 59 | 29,42 | 59 | 0,048 | SSW | 2 | Fair. |
| | 61 | 2 | 0 | 61 | 60 | 29,31 | 53 | | SSW | 2 | Cloudy. |
| 13 | 46 | 7 | 0 | 52 | 59 | 29,51 | 60 | 0,250 | WSW | 1 | Cloudy. |
| | 63 | 2 | 0 | 62 | 60 | 29,50 | 48 | | WSW | 1 | Rain. |
| 14 | 49 | 7 | 0 | 51 | 59 | 29,32 | 61 | 0,168 | SSE | 2 | Fair. |
| | 56 | 2 | 0 | 56 | 59 | 29,11 | 65 | | SSE | 2 | Rain. |
| 15 | 51 | 7 | 0 | 52 | 58 | 29,50 | 64 | 0,165 | SSE | 1 | Rain. |
| | 68 | 2 | 0 | 67 | 61 | 29,49 | 53 | | SSE | 2 | Fair. |
| 16 | 52 | 7 | 0 | 53 | 60 | 29,64 | 60 | 0,045 | SSE | 1 | Rain. |
| | 65 | 2 | 0 | 64 | 60 | 29,67 | 61 | | ENE | 1 | Cloudy. |

## METEOROLOGICAL JOURNAL
## for May, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| May 17 | 53 | 7 | 0 | 54 | 60 | 29,79 | 63 | 0,265 | S | 2 | Cloudy. |
| | 64 | 2 | 0 | 64 | 61 | 29,96 | 45 | | S | 2 | Fair. |
| 18 | 50 | 7 | 0 | 52 | 60 | 30,18 | 61 | | SW | 1 | Cloudy. |
| | 66 | 2 | 0 | 65 | 62 | 30,18 | 38 | | WNW | 1 | Fair. |
| 19 | 50 | 7 | 0 | 53 | 60 | 30,23 | 57 | | SW | 1 | Cloudy. |
| | 64 | 2 | 0 | 62 | 60 | 30,15 | 47 | | SW | 1 | Cloudy. |
| 20 | 52 | 7 | 0 | 54 | 60 | 30,16 | 53 | | SSW | 1 | Cloudy. |
| | 62 | 2 | 0 | 62 | 61 | 30,24 | 38 | | NW | 1 | Cloudy. |
| 21 | 46 | 7 | 0 | 51 | 60 | 30,42 | 52 | | WNW | 1 | Fine. |
| | 66 | 2 | 0 | 66 | 61 | 30,44 | 43 | | NW | 1 | Fair. |
| 22 | 47 | 7 | 0 | 50 | 60 | 30,44 | 56 | | ESE | 1 | Cloudy. |
| | 65 | 2 | 0 | 65 | 61 | 30,41 | 38 | | ENE | 1 | Fine. |
| 23 | 45 | 7 | 0 | 49 | 60 | 30,41 | 57 | | NE | 1 | Fine. |
| | 64 | 2 | 0 | 64 | 61 | 30,38 | 40 | | E | 1 | Fine. |
| 24 | 46 | 7 | 0 | 51 | 60 | 30,32 | 53 | | E | 1 | Fine. |
| | 67 | 2 | 0 | 66 | 61 | 30,27 | 30 | | E | 1 | Hazy. |
| 25 | 51 | 7 | 0 | 54 | 61 | 30,18 | 50 | | S | 1 | Hazy. |
| | 73 | 2 | 0 | 72 | 63 | 30,14 | 37 | | E | 1 | Fine. |
| 26 | 50 | 7 | 0 | 54 | 61 | 30,13 | 52 | | E | 1 | Fine. |
| | 72 | 2 | 0 | 71 | 63 | 30,13 | 36 | | E | 1 | Fine. |
| 27 | 53 | 7 | 0 | 56 | 63 | 30,20 | 60 | | E | 1 | Fine. |
| | 76 | 2 | 0 | 75 | 64 | 30,16 | 34 | | E | 1 | Fine. |
| 28 | 54 | 7 | 0 | 58 | 64 | 30,11 | 47 | | NE | 1 | Fine. |
| | 73 | 2 | 0 | 73 | 66 | 30,01 | 32 | | NE | 2 | Fine. |
| 29 | 50 | 7 | 0 | 54 | 64 | 30,09 | 47 | | NE | 2 | Cloudy. |
| | 66 | 2 | 0 | 66 | 64 | 30,09 | 39 | | NE | 2 | Cloudy. |
| 30 | 49 | 7 | 0 | 53 | 64 | 30,09 | 54 | | NE | 2 | Cloudy. |
| | 60 | 2 | 0 | 54 | 64 | 30,05 | 60 | | N | 1 | Rain. |
| 31 | 51 | 7 | 0 | 54 | 64 | 29,92 | 65 | 0,205 | NE | 1 | Cloudy. |
| | 61 | 2 | 0 | 61 | 63 | 29,87 | 62 | | NE | 1 | Cloudy. |

## METEOROLOGICAL JOURNAL

## for June, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| June 1 | 53 | 7 | 0 | 54 | 63 | 29,80 | 69 | | NE | 1 | Cloudy. |
| | 68 | 2 | 0 | 68 | 63 | 29,77 | 54 | | NE | 1 | Cloudy. |
| 2 | 57 | 7 | 0 | 57 | 63 | 29,78 | 65 | | S | 2 | Cloudy. |
| | 69 | 2 | 0 | 68 | 63 | 29,84 | 51 | | S | 1 | Cloudy. |
| 3 | 48 | 7 | 0 | 51 | 63 | 30,10 | 58 | 0,035 | S | 1 | Fair. |
| | 70 | 2 | 0 | 69 | 63 | 30,13 | 36 | | SW | 1 | Cloudy. |
| 4 | 57 | 7 | 0 | 58 | 62 | 30,18 | 61 | | S | 2 | Cloudy. |
| | 73 | 2 | 0 | 71 | 63 | 30,23 | 49 | | SSW | 2 | Cloudy. |
| 5 | 57 | 7 | 0 | 57 | 63 | 30,28 | 67 | | W | 1 | Cloudy. |
| | 73 | 2 | 0 | 71 | 64 | 30,30 | 52 | | S | 1 | Hazy. |
| 6 | 56 | 7 | 0 | 58 | 64 | 30,32 | 63 | | E | 1 | Cloudy. |
| | 74 | 2 | 0 | 74 | 66 | 30,30 | 48 | | E | 1 | Fine. |
| 7 | 59 | 7 | 0 | 63 | 66 | 30,26 | 67 | | E | 1 | Fine. |
| | 82 | 2 | 0 | 82 | 68 | 30,27 | 41 | | ENE | 1 | Fine. |
| 8 | 52 | 7 | 0 | 57 | 66 | 30,42 | 52 | | ENE | 2 | Cloudy. |
| | 69 | 2 | 0 | 69 | 67 | 30,38 | 43 | | NE | 1 | Fine. |
| 9 | 48 | 7 | 0 | 52 | 65 | 30,34 | 55 | | NE | 1 | Fine. |
| | 68 | 2 | 0 | 67 | 67 | 30,31 | 42 | | E | 1 | Fine. |
| 10 | 52 | 7 | 0 | 57 | 65 | 30,36 | 52 | | NE | 1 | Cloudy. |
| | 67 | 2 | 0 | 67 | 65 | 30,35 | 43 | | NE | 1 | Cloudy. |
| 11 | 47 | 7 | 0 | 53 | 64 | 30,39 | 52 | | NE | 1 | Fair. |
| | 67 | 2 | 0 | 67 | 64 | 30,37 | 42 | | NE | 1 | Cloudy. |
| 12 | 48 | 7 | 0 | 53 | 63 | 30,33 | 52 | | E | 1 | Fine. |
| | 70 | 2 | 0 | 70 | 65 | 30,27 | 39 | | E | 1 | Fine. |
| 13 | 53 | 7 | 0 | 58 | 64 | 30,23 | 53 | | S | 1 | Fine. |
| | 78 | 2 | 0 | 77 | 67 | 30,21 | 36 | | SSE | 1 | Fine. |
| 14 | 57 | 7 | 0 | 61 | 66 | 30,18 | 48 | | E | 1 | Fine. |
| | 75 | 2 | 0 | 75 | 68 | 30,15 | 42 | | E | 1 | Fine. |
| 15 | 56 | 7 | 0 | 61 | 67 | 30,05 | 55 | | E | 1 | Fine. |
| | 72 | 2 | 0 | 71 | 67 | 30,00 | 40 | | E | 1 | Fine. |
| 16 | 56 | 7 | 0 | 60 | 67 | 29,95 | 62 | | NE | 1 | Cloudy. |
| | 74 | 2 | 0 | 73 | 68 | 29,93 | 42 | | E | 1 | Fair. |

## METEOROLOGICAL JOURNAL

## for June, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| June 17 | 56 | 7 | 0 | 58 | 67 | 29,91 | 63 | | ENE | 1 | Cloudy. |
| | 77 | 2 | 0 | 76 | 68 | 29,89 | 40 | | NE | 1 | Fair. |
| 18 | 56 | 7 | 0 | 60 | 68 | 29,89 | 59 | | NE | 1 | Fair. |
| | 79 | 2 | 0 | 79 | 69 | 29,84 | 38 | | S | 1 | Fair. |
| 19 | 58 | 7 | 0 | 58 | 68 | 29,75 | 58 | 0,097 | S | 2 | Fair. |
| | 62 | 2 | 0 | 59 | 67 | 29,71 | 54 | | W | 2 | Rain. |
| 20 | 48 | 7 | 0 | 52 | 66 | 29,65 | 58 | 0,435 | WSW | 2 | Fine. |
| | 70 | 2 | 0 | 70 | 66 | 29,70 | 42 | | WNW | 1 | Cloudy. |
| 21 | 54 | 7 | 0 | 54 | 65 | 29,73 | 54 | 0,105 | WNW | 2 | Fair. |
| | 63 | 2 | 0 | 61 | 65 | 29,78 | 55 | | WNW | 2 | Rain. |
| 22 | 53 | 7 | 0 | 56 | 64 | 29,98 | 51 | 0,017 | WNW | 2 | Cloudy. |
| | 66 | 2 | 0 | 66 | 65 | 30,06 | 44 | | NW | 1 | Cloudy. |
| 23 | 53 | 7 | 0 | 55 | 64 | 30,17 | 51 | | W | 1 | Cloudy. |
| | 69 | 2 | 0 | 67 | 64 | 30,17 | 52 | | S | 1 | Cloudy |
| 24 | 58 | 7 | 0 | 59 | 64 | 30,26 | 62 | 0,253 | NE | 1 | Fine. |
| | 75 | 2 | 0 | 75 | 65 | 30,23 | 42 | | NE | 1 | Cloudy. |
| 25 | 57 | 7 | 0 | 59 | 64 | 30,13 | 62 | | SW | 1 | Fair. |
| | 79 | 2 | 0 | 77 | 66 | 30,07 | 41 | | WNW | 1 | Fair. |
| 26 | 59 | 7 | 0 | 61 | 66 | 30,03 | 59 | | W | 1 | Cloudy. |
| | 76 | 2 | 0 | 75 | 67 | 30,01 | 42 | | WNW | 1 | Cloudy. |
| 27 | 61 | 7 | 0 | 62 | 66 | 30,02 | 56 | | WSW | 1 | Fine. |
| | 69 | 2 | 0 | 68 | 68 | 30,00 | 35 | | N | 1 | Fine. |
| 28 | 61 | 7 | 0 | 62 | 68 | 30,03 | 48 | | WSW | 1 | Fine. |
| | 86 | 2 | 0 | 86 | 70 | 29,98 | 32 | | SSW | 1 | Fine. |
| 29 | 65 | 7 | 0 | 68 | 70 | 29,85 | 42 | | SE | 1 | Fine. |
| | 83 | 2 | 0 | 81 | 71 | 29,77 | 40 | | S | 2 | Cloudy. |
| 30 | 61 | 7 | 0 | 62 | 70 | 29,78 | 56 | 0,018 | SW | 2 | Fair. |
| | 81 | 2 | 0 | 81 | 70 | 29,82 | 41 | | W | 2 | Cloudy. |

## METEOROLOGICAL JOURNAL

## for July, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| July 1 | 59 | 7 | 0 | 60 | 68 | 29,85 | 59 | 0,105 | S | 2 | Cloudy. |
| | 74 | 2 | 0 | 73 | 68 | 29,72 | 50 | | S | 2 | Fair. |
| 2 | 55 | 7 | 0 | 58 | 68 | 29,62 | 52 | | S | 2 | Fair. |
| | 69 | 2 | 0 | 62 | 67 | 29,63 | 54 | | W | 2 | Rain. |
| 3 | 54 | 7 | 0 | 56 | 67 | 29,85 | 57 | 0,210 | SW | 1 | Fine. |
| | 76 | 2 | 0 | 76 | 68 | 29,83 | 46 | | SSW | 1 | Fair. |
| 4 | 63 | 7 | 0 | 64 | 68 | 29,83 | 66 | 0,025 | SW | 2 | Cloudy. |
| | 78 | 2 | 0 | 78 | 69 | 29,83 | 44 | | SSW | 2 | Cloudy. |
| 5 | 63 | 7 | 0 | 64 | 68 | 29,80 | 58 | | S | 2 | Cloudy. |
| | 70 | 2 | 0 | 69 | 68 | 29,76 | 59 | | SSE | 2 | Cloudy. |
| 6 | 59 | 7 | 0 | 60 | 68 | 29,78 | 63 | 0,396 | SSW | 1 | Fair. |
| | 73 | 2 | 0 | 71 | 68 | 29,79 | 45 | | SW | 1 | Fair. |
| 7 | 58 | 7 | 0 | 61 | 65 | 29,77 | 59 | | S | 2 | Cloudy. |
| | 73 | 2 | 0 | 73 | 67 | 29,76 | 43 | | S | 2 | Cloudy. |
| 8 | 57 | 7 | 0 | 60 | 66 | 29,80 | 56 | | SSW | 1 | Fair. |
| | 73 | 2 | 0 | 73 | 67 | 29,74 | 44 | | SSW | 1 | Cloudy. |
| 9 | 53 | 7 | 0 | 57 | 66 | 29,83 | 58 | 0,083 | SSW | 1 | Cloudy. |
| | 71 | 2 | 0 | 71 | 67 | 29,86 | 52 | | SW | 2 | Cloudy. |
| 10 | 61 | 7 | 0 | 63 | 66 | 29,85 | 71 | 0,102 | S | 1 | Rain. |
| | 73 | 2 | 0 | 73 | 67 | 29,88 | 57 | | S | 1 | Cloudy. |
| 11 | 59 | 7 | 0 | 59 | 67 | 29,72 | 67 | 0,145 | S | 1 | Rain. |
| | 73 | 2 | 0 | 72 | 68 | 29,68 | 44 | | W | 1 | Cloudy. |
| 12 | 53 | 7 | 0 | 56 | 65 | 29,73 | 58 | | SW | 2 | Fair. |
| | 67 | 2 | 0 | 62 | 65 | 29,71 | 53 | | SSW | 1 | Cloudy. |
| 13 | 51 | 7 | 0 | 54 | 64 | 29,83 | 64 | 0,083 | S | 1 | Fine. |
| | 70 | 2 | 0 | 68 | 65 | 29,80 | 50 | | SW | 2 | Fair. |
| 14 | 56 | 7 | 0 | 59 | 65 | 29,48 | 67 | 0,150 | S | 2 | Rain. |
| | 70 | 2 | 0 | 70 | 66 | 29,56 | 41 | | SW | 2 | Fair. |
| 15 | 52 | 7 | 0 | 55 | 65 | 29,67 | 57 | 0,078 | SSW | 2 | Fine. |
| | 69 | 2 | 0 | 68 | 65 | 29,64 | 46 | | S | 2 | Fair. |
| 16 | 54 | 7 | 0 | 57 | 64 | 29,44 | 57 | | S | 2 | Fair. |
| | 70 | 2 | 0 | 69 | 65 | 29,36 | 47 | | S | 2 | Fair. |

## METEOROLOGICAL JOURNAL
## for July, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| July 17 | 52 | 7 | 0 | 57 | 64 | 29,38 | 60 | 0,025 | NE | 1 | Hazy. |
| | 69 | 2 | 0 | 67 | 65 | 29,49 | 50 | | NE | 1 | Hazy. |
| 18 | 52 | 7 | 0 | 56 | 64 | 29,90 | 62 | 0,120 | SW | 1 | Fair. |
| | 74 | 2 | 0 | 74 | 66 | 29,96 | 38 | | W | 1 | Fair. |
| 19 | 58 | 7 | 0 | 60 | 65 | 29,94 | 72 | 0,137 | SW | 1 | Rain. |
| | 75 | 2 | 0 | 74 | 67 | 29,94 | 50 | | S | 1 | Cloudy. |
| 20 | 59 | 7 | 0 | 61 | 66 | 29,64 | 74 | 0,360 | S | 2 | Cloudy. |
| | 72 | 2 | 0 | 72 | 67 | 29,49 | 68 | | S | 2 | Rain. |
| 21 | 53 | 7 | 0 | 55 | 66 | 29,58 | 60 | 0,110 | SSW | 2 | Fair. |
| | 71 | 2 | 0 | 69 | 66 | 29,64 | 46 | | SSW | 2 | Fair. |
| 22 | 53 | 7 | 0 | 54 | 65 | 29,77 | 61 | | S | 1 | Fine. |
| | 71 | 2 | 0 | 70 | 66 | 29,83 | 44 | | SSW | 2 | Fair. |
| 23 | 52 | 7 | 0 | 56 | 64 | 29,84 | 62 | 0,140 | SE | 2 | Fair. |
| | 70 | 2 | 0 | 69 | 65 | 29,78 | 46 | | SSE | 2 | Cloudy. |
| 24 | 56 | 7 | 0 | 58 | 64 | 29,63 | 64 | 0,035 | SSE | 1 | Cloudy. |
| | 72 | 2 | 0 | 69 | 65 | 29,64 | 53 | | SE | 1 | Cloudy. |
| 25 | 58 | 7 | 0 | 56 | 64 | 29,78 | 68 | 0,365 | SW | 1 | Cloudy. |
| | 68 | 2 | 0 | 67 | 65 | 29,86 | 54 | | NW | 1 | Cloudy. |
| 26 | 54 | 7 | 0 | 56 | 64 | 30,01 | 65 | 0,195 | WSW | 1 | Cloudy. |
| | 68 | 2 | 0 | 66 | 64 | 30,05 | 52 | | W | 1 | Cloudy. |
| 27 | 57 | 7 | 0 | 60 | 64 | 30,11 | 67 | | WNW | 1 | Cloudy. |
| | 72 | 2 | 0 | 72 | 65 | 30,14 | 52 | | WNW | 1 | Cloudy. |
| 28 | 56 | 7 | 0 | 57 | 64 | 30,16 | 67 | | SSW | 1 | Cloudy. |
| | 70 | 2 | 0 | 70 | 64 | 30,14 | 53 | | SSW | 1 | Cloudy. |
| 29 | 60 | 7 | 0 | 59 | 64 | 30,13 | 69 | 0,015 | W | 1 | Cloudy. |
| | 71 | 2 | 0 | 71 | 66 | 30,16 | 42 | | N | 1 | Fair. |
| 30 | 56 | 7 | 0 | 60 | 64 | 30,17 | 64 | | NE | 1 | Cloudy. |
| | 74 | 2 | 0 | 74 | 67 | 30,10 | 45 | | S | 1 | Hazy. |
| 31 | 56 | 7 | 0 | 58 | 65 | 30,02 | 63 | | S | 1 | Fair. |
| | 75 | 2 | 0 | 75 | 72 | 29,97 | 42 | | SE | 2 | Fine. |

## METEOROLOGICAL JOURNAL

## for August, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hy-gro-me-ter. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| Aug. 1 | 61 | 7 | 0 | 64 | 68 | 29,86 | 55 | | E | 1 | Cloudy. |
| | 83 | 2 | 0 | 82 | 70 | 29,85 | 42 | | SSE | 1 | Cloudy. |
| 2 | 64 | 7 | 0 | 64 | 68 | 29.91 | 64 | 0,415 | SW | 2 | Cloudy. |
| | 73 | 2 | 0 | 73 | 69 | 29,98 | 45 | | W | 2 | Cloudy. |
| 3 | 58 | 7 | 0 | 60 | 68 | 30,07 | 60 | | E | 1 | Fair. |
| | 76 | 2 | 0 | 76 | 70 | 29,99 | 46 | | SE | 1 | Fine. |
| 4 | 65 | 7 | 0 | 65 | 69 | 29,70 | 72 | 0,380 | S | 1 | Rain. |
| | 74 | 2 | 0 | 73 | 70 | 29,75 | 55 | | SSW | 1 | Cloudy. |
| 5 | 56 | 7 | 0 | 57 | 68 | 30,11 | 62 | 0,125 | NW | 1 | Fine. |
| | 70 | 2 | 0 | 70 | 67 | 30,20 | 45 | | NW | 1 | Cloudy. |
| 6 | 53 | 7 | 0 | 56 | 67 | 30,30 | 61 | | W | 1 | Cloudy. |
| | 71 | 2 | 0 | 70 | 67 | 30,32 | 53 | | W | 1 | Cloudy. |
| 7 | 56 | 7 | 0 | 57 | 67 | 30,30 | 60 | | NE | 1 | Hazy. |
| | 73 | 2 | 0 | 72 | 68 | 30,26 | 47 | | NE | 1 | Cloudy. |
| 8 | 61 | 7 | 0 | 61 | 67 | 30,17 | 62 | | E | 1 | Cloudy. |
| | 67 | 2 | 0 | 66 | 67 | 30,11 | 52 | | E | 1 | Cloudy. |
| 9 | 55 | 7 | 0 | 57 | 66 | 29,97 | 61 | | SE | 1 | Fair. |
| | 76 | 2 | 0 | 76 | 67 | 29,94 | 51 | | SSE | 1 | Hazy. |
| 10 | 58 | 7 | 0 | 58 | 66 | 29,86 | 64 | 0,063 | SSE | 1 | Rain. |
| | 68 | 2 | 0 | 66 | 66 | 29,80 | 60 | | SE | 2 | Cloudy. |
| 11 | 54 | 7 | 0 | 56 | 66 | 29,97 | 67 | 0,072 | WSW | 1 | Fair. |
| | 73 | 2 | 0 | 73 | 67 | 30,03 | 45 | | W | 1 | Fair. |
| 12 | 61 | 7 | 0 | 62 | 66 | 30,02 | 68 | 0,025 | S | 2 | Cloudy. |
| | 76 | 2 | 0 | 76 | 68 | 29,98 | 57 | | S | 2 | Fair. |
| 13 | 61 | 7 | 0 | 64 | 68 | 29,88 | 68 | | S | 2 | Cloudy. |
| | 80 | 2 | 0 | 80 | 70 | 29,82 | 50 | | S | 2 | Fine. |
| 14 | 64 | 7 | 0 | 64 | 69 | 29,77 | 64 | 0,020 | NNE | 1 | Cloudy. |
| | 74 | 2 | 0 | 74 | 70 | 29,83 | 43 | | NE | 1 | Fair. |
| 15 | 56 | 7 | 0 | 58 | 68 | 29,95 | 64 | 0,223 | NE | 1 | Cloudy. |
| | 72 | 2 | 0 | 70 | 68 | 29,98 | 46 | | N | 1 | Fair. |
| 16 | 55 | 7 | 0 | 59 | 68 | 30,06 | 61 | | NE | 1 | Fair. |
| | 70 | 2 | 0 | 69 | 69 | 30,08 | 50 | | E | 1 | Fair. |

## METEOROLOGICAL JOURNAL

## for August, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| Aug. 17 | 54 | 7 | 0 | 56 | 68 | 30,23 | 61 | 0,100 | E | 1 | Fair. |
| | 74 | 2 | 0 | 74 | 69 | 30,25 | 44 | | E | 1 | Fine. |
| 18 | 55 | 7 | 0 | 58 | 66 | 30,22 | 61 | | E | 1 | Fair. |
| | 74 | 2 | 0 | 74 | 69 | 30,16 | 44 | | E | 1 | Fine. |
| 19 | 60 | 7 | 0 | 63 | 69 | 30,06 | 58 | | E | 1 | Cloudy. |
| | 76 | 2 | 0 | 76 | 70 | 30,05 | 43 | | ESE | 1 | Fair. |
| 20 | 59 | 7 | 0 | 61 | 69 | 30,15 | 61 | | ENE | 1 | Fair. |
| | 75 | 2 | 0 | 75 | 70 | 30,20 | 44 | | E | 1 | Fine. |
| 21 | 58 | 7 | 0 | 62 | 69 | 30,24 | 70 | | NE | 1 | Cloudy. |
| | 75 | 2 | 0 | 75 | 71 | 30,24 | 48 | | E | 1 | Fine. |
| 22 | 59 | 7 | 0 | 63 | 70 | 30,09 | 70 | | E | 1 | Fine. |
| | 76 | 2 | 0 | 76 | 72 | 30,01 | 41 | | E | 1 | Fine. |
| 23 | 62 | 7 | 0 | 64 | 71 | 29,80 | 61 | | NE | 1 | Cloudy. |
| | 76 | 2 | 0 | 76 | 71 | 29,76 | 55 | | SSW | 1 | Cloudy. |
| 24 | 56 | 7 | 0 | 58 | 70 | 30,03 | 56 | 0,102 | SW | 1 | Cloudy. |
| | 71 | 2 | 0 | 70 | 70 | 30,12 | 41 | | W | 1 | Fair. |
| 25 | 58 | 7 | 0 | 62 | 68 | 30,11 | 62 | | SW | 2 | Cloudy. |
| | 75 | 2 | 0 | 73 | 70 | 30,13 | 47 | | WNW | 2 | Cloudy. |
| 26 | 52 | 7 | 0 | 55 | 68 | 30,35 | 58 | | NE | 1 | Fine. |
| | 70 | 2 | 0 | 70 | 70 | 30,35 | 42 | | NE | 1 | Fine. |
| 27 | 54 | 7 | 0 | 57 | 68 | 30,35 | 55 | | W | 1 | Cloudy. |
| | 71 | 2 | 0 | 71 | 68 | 30,35 | 47 | | NNE | 1 | Cloudy. |
| 28 | 62 | 7 | 0 | 62 | 68 | 30,33 | 59 | | N | 1 | Cloudy. |
| | 71 | 2 | 0 | 70 | 69 | 30,32 | 47 | | N | 1 | Fair. |
| 29 | 60 | 7 | 0 | 63 | 68 | 30,30 | 59 | | NE | 1 | Cloudy. |
| | 73 | 2 | 0 | 72 | 70 | 30,25 | 43 | | E | 1 | Fine. |
| 30 | 53 | 7 | 0 | 56 | 68 | 30,30 | 60 | | E | 1 | Fine. |
| | 71 | 2 | 0 | 71 | 70 | 30,28 | 42 | | E | 1 | Fine. |
| 31 | 58 | 7 | 0 | 58 | 67 | 30,34 | 55 | | ENE | 1 | Hazy. |
| | 67 | 2 | 0 | 67 | 70 | 30,30 | 44 | | E | 1 | Fine. |

## METEOROLOGICAL JOURNAL
## for September, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hy-gro-me-ter. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| Sept. 1 | 54 | 7 | 0 | 57 | 68 | 30,26 | 56 | | SE | 1 | Fine. |
| | 72 | 2 | 0 | 72 | 69 | 30,18 | 44 | | S | 1 | Fine. |
| 2 | 54 | 7 | 0 | 60 | 68 | 30,00 | 54 | | SSE | 1 | Fine. |
| | 76 | 2 | 0 | 75 | 69 | 29,90 | 37 | | S | 2 | Fair. |
| 3 | 60 | 7 | 0 | 63 | 68 | 29,76 | 64 | | S | 1 | Fair. |
| | 74 | 2 | 0 | 73 | 69 | 29,70 | 55 | | SSW | 2 | Cloudy. |
| 4 | 61 | 7 | 0 | 63 | 69 | 29,56 | 61 | | SW | 2 | Cloudy. |
| | 73 | 2 | 0 | 73 | 70 | 29,70 | 43 | | WSW | 2 | Fair. |
| 5 | 55 | 7 | 0 | 58 | 68 | 29,88 | 57 | | S | 2 | Cloudy. |
| | 68 | 2 | 0 | 68 | 68 | 29,87 | 53 | | S | 2 | Fair. |
| 6 | 50 | 7 | 0 | 51 | 67 | 30,01 | 59 | 0,035 | S | 1 | Cloudy. |
| | 64 | 2 | 0 | 64 | 66 | 29,97 | 49 | | SE | 1 | Cloudy. |
| 7 | 54 | 7 | 0 | 54 | 66 | 29,85 | 60 | 0,028 | NE | 1 | Rain. |
| | 63 | 2 | 0 | 62 | 65 | 29,78 | 54 | | NE | 1 | Cloudy. |
| 8 | 55 | 7 | 0 | 57 | 65 | 29,67 | 70 | 0,023 | NE | 1 | Cloudy. |
| | 65 | 2 | 0 | 65 | 65 | 29,66 | 56 | | NNE | 1 | Cloudy. |
| 9 | 53 | 7 | 0 | 54 | 64 | 29,72 | 65 | | NE | 2 | Cloudy. |
| | 68 | 2 | 0 | 68 | 66 | 29,65 | 58 | | ESE | 2 | Fair. |
| 10 | 60 | 7 | 0 | 62 | 66 | 29,53 | 63 | | S | 2 | Fair. |
| | 67 | 2 | 0 | 65 | 66 | 29,54 | 61 | | S | 2 | Cloudy. |
| 11 | 53 | 7 | 0 | 53 | 65 | 29,51 | 68 | 0,475 | SSW | 2 | Fine. |
| | 67 | 2 | 0 | 65 | 66 | 29,49 | 49 | | S | 2 | Cloudy. |
| 12 | 49 | 7 | 0 | 52 | 63 | 29,27 | 65 | 0,285 | S | 2 | Rain. [Much wind last night. |
| | 62 | 2 | 0 | 60 | 64 | 29,30 | 54 | | S | 2 | Fair. |
| 13 | 49 | 7 | 0 | 51 | 62 | 29,16 | 70 | 0,270 | SSE | 2 | Rain. |
| | 60 | 2 | 0 | 60 | 63 | 29,32 | 61 | | NE | 2 | Fair. |
| 14 | 53 | 7 | 0 | 55 | 62 | 29,90 | 66 | 0,112 | N | 1 | Fair. |
| | 64 | 2 | 0 | 64 | 63 | 30,03 | 59 | | NNE | 1 | Cloudy. |
| 15 | 54 | 7 | 0 | 56 | 62 | 30,08 | 67 | | S | 1 | Cloudy. |
| | 63 | 2 | 0 | 65 | 63 | 30,02 | 70 | | SSW | 1 | Cloudy. |
| 16 | 55 | 7 | 0 | 55 | 63 | 30,06 | 68 | 0,043 | WSW | 1 | Fine. |
| | 67 | 2 | 0 | 66 | 64 | 30,13 | 46 | | W | 1 | Fine. |

## METEOROLOGICAL JOURNAL

## for September, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| Sept. 17 | 52 | 7 | 0 | 53 | 63 | 30,19 | 65 | | W | 1 | Fair. |
| | 68 | 2 | 0 | 68 | 64 | 30,17 | 48 | | W | 1 | Cloudy. |
| 18 | 52 | 7 | 0 | 53 | 63 | 30,20 | 68 | | W | 1 | Fine. |
| | 69 | 2 | 0 | 68 | 64 | 30,19 | 52 | | SSW | 1 | Fair. |
| 19 | 52 | 7 | 0 | 53 | 63 | 30,09 | 65 | | E | 1 | Cloudy. |
| | 67 | 2 | 0 | 67 | 65 | 30,06 | 60 | | S | 1 | Fair. |
| 20 | 53 | 7 | 0 | 54 | 64 | 29,96 | 67 | | E | 1 | Cloudy. |
| | 68 | 2 | 0 | 68 | 65 | 29,82 | 55 | | S | 2 | Fair. |
| 21 | 50 | 7 | 0 | 50 | 64 | 29,86 | 62 | | WSW | 1 | Fair. |
| | 64 | 2 | 0 | 64 | 65 | 29,88 | 45 | | W | 1 | Fair. |
| 22 | 48 | 7 | 0 | 52 | 63 | 30,03 | 63 | | WSW | 1 | Cloudy. |
| | 64 | 2 | 0 | 64 | 63 | 30,05 | 56 | | SSW | 1 | Cloudy. |
| 23 | 55 | 7 | 0 | 58 | 63 | 30,06 | 69 | | S | 2 | Cloudy. |
| | 68 | 2 | 0 | 68 | 64 | 30,05 | 49 | | S | 2 | Fine. |
| 24 | 59 | 7 | 0 | 61 | 65 | 29,95 | 73 | | S | 1 | Cloudy. |
| | 70 | 2 | 0 | 70 | 65 | 29,99 | 55 | | S | 1 | Cloudy. |
| 25 | 52 | 7 | 0 | 53 | 64 | 29,92 | 61 | | SSW | 1 | Cloudy. |
| | 62 | 2 | 0 | 62 | 64 | 29,90 | 41 | | W | 2 | Fair. |
| 26 | 48 | 7 | 0 | 51 | 63 | 29,94 | 62 | | SW | 1 | Cloudy. |
| | 61 | 2 | 0 | 61 | 64 | 29,91 | 53 | | WNW | 2 | Fair. |
| 27 | 50 | 7 | 0 | 51 | 62 | 29,24 | 67 | 0,115 | S | 1 | Cloudy. |
| | 55 | 2 | 0 | 52 | 61 | 28,97 | 66 | | W | 1 | Rain. |
| 28 | 45 | 7 | 0 | 45 | 60 | 28,97 | 66 | 0,730 | WNW | 2 | Rain. |
| | 50 | 2 | 0 | 50 | 58 | 29,01 | 65 | | NW | 2 | Rain. |
| 29 | 44 | 7 | 0 | 48 | 58 | 29,16 | 64 | 0,321 | W | 2 | Fair. |
| | 56 | 2 | 0 | 56 | 58 | 29,25 | 59 | | W | 2 | Cloudy. |
| 30 | 47 | 7 | 0 | 47 | 58 | 29,55 | 69 | | WSW | 1 | Cloudy. |
| | 58 | 2 | 0 | 57 | 59 | 29,73 | 59 | | SW | 1 | Fair. |

## METEOROLOGICAL JOURNAL

## for October, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| Oct. 1 | 41 | 7 | 0 | 43 | 57 | 30,07 | 70 | | W | 1 | Cloudy. |
| | 58 | 2 | 0 | 58 | 58 | 30,11 | 49 | | W | 1 | Fair. |
| 2 | 52 | 7 | 0 | 54 | 58 | 30,19 | 66 | | WNW | 1 | Cloudy. |
| | 61 | 2 | 0 | 61 | 60 | 30,26 | 56 | | N | 1 | Fair. |
| 3 | 46 | 7 | 0 | 48 | 59 | 30,30 | 69 | | S | 1 | Foggy. |
| | 59 | 2 | 0 | 59 | 61 | 30,30 | 62 | | NE | 1 | Fine. |
| 4 | 52 | 7 | 0 | 54 | 60 | 30,16 | 71 | | NE | 1 | Cloudy. |
| | 58 | 2 | 0 | 58 | 62 | 30,10 | 50 | | E | 2 | Fine. |
| 5 | 49 | 7 | 0 | 54 | 60 | 29,88 | 66 | | E | 2 | Cloudy. |
| | 58 | 2 | 0 | 57 | 61 | 29,83 | 65 | | E | 2 | Cloudy. |
| 6 | 52 | 7 | 0 | 53 | 60 | 29,98 | 80 | | E | 1 | Foggy. |
| | 59 | 2 | 0 | 59 | 61 | 30,07 | 71 | | S | 1 | Cloudy. |
| 7 | 46 | 7 | 0 | 46 | 60 | 30,26 | 73 | | S | 1 | Fine. |
| | 63 | 2 | 0 | 63 | 61 | 30,26 | 63 | | SW | 1 | Fine. |
| 8 | 47 | 7 | 0 | 47 | 60 | 30,26 | 70 | | SW | 1 | Fine. |
| | 62 | 2 | 0 | 62 | 63 | 30,26 | 62 | | SSW | 1 | Fine. |
| 9 | 46 | 7 | 0 | 50 | 61 | 30,17 | 70 | | W | 1 | Cloudy. |
| | 62 | 2 | 0 | 62 | 63 | 30,08 | 58 | | SSE | 1 | Fair. |
| 10 | 53 | 7 | 0 | 55 | 62 | 29,88 | 77 | | S | 2 | Cloudy. |
| | 64 | 2 | 0 | 62 | 62 | 29,82 | 68 | | S | 2 | Cloudy. |
| 11 | 55 | 7 | 0 | 55 | 62 | 29,74 | 70 | 0,140 | SSW | 1 | Cloudy. |
| | 60 | 2 | 0 | 55 | 62 | 29,70 | 68 | | NW | 1 | Rain. |
| 12 | 40 | 7 | 0 | 40 | 60 | 30,07 | 65 | 0,137 | NW | 1 | Fine. |
| | 52 | 2 | 0 | 51 | 60 | 30,19 | 52 | | NW | 1 | Cloudy. |
| 13 | 32 | 7 | 0 | 33 | 58 | 30,39 | 62 | | W | 1 | Fine. |
| | 47 | 2 | 0 | 47 | 60 | 30,35 | 45 | | S | 1 | Fine. |
| 14 | 37 | 7 | 0 | 37 | 58 | 30,04 | 60 | | E | 1 | Fine. |
| | 55 | 2 | 0 | 52 | 59 | 29,82 | 56 | | ESE | 2 | Fair. |
| 15 | 52 | 7 | 0 | 56 | 59 | 29,63 | 82 | 0,051 | S | 2 | Cloudy. |
| | 60 | 2 | 0 | 60 | 62 | 29,81 | 51 | | S | 2 | Fair. |
| 16 | 50 | 7 | 0 | 50 | 60 | 29,78 | 70 | | S | 1 | Cloudy. |
| | 57 | 2 | 0 | 55 | 62 | 29,85 | 70 | | S | 1 | Cloudy. |

## METEOROLOGICAL JOURNAL

## for October, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hy-gro-me-ter. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| Oct. 17 | 47 | 7 | 0 | 53 | 60 | 29,92 | 73 | 0,158 | SSE | 2 | Cloudy. |
| | 62 | 2 | 0 | 62 | 62 | 29,92 | 70 | | SSE | 2 | Cloudy. |
| 18 | 56 | 7 | 0 | 56 | 62 | 29,72 | 74 | 0,018 | SE | 2 | Cloudy. |
| | 58 | 2 | 0 | 56 | 62 | 29,64 | 72 | | SSE | 2 | Rain. |
| 19 | 42 | 7 | 0 | 42 | 60 | 29,81 | 72 | 0,265 | SW | 1 | Fair. |
| | 56 | 2 | 0 | 56 | 63 | 29,89 | 52 | | WNW | 1 | Fair. |
| 20 | 44 | 7 | 0 | 46 | 60 | 30,05 | 69 | | SSW | 1 | Cloudy. |
| | 58 | 2 | 0 | 58 | 62 | 30,04 | 63 | | S | 1 | Cloudy. |
| 21 | 54 | 7 | 0 | 55 | 61 | 30,07 | 73 | | SSW | 1 | Cloudy. |
| | 60 | 2 | 0 | 60 | 63 | 30,08 | 60 | | S | 2 | Fair. |
| 22 | 53 | 7 | 0 | 53 | 60 | 29,90 | 64 | | S | 2 | Cloudy. |
| | 59 | 2 | 0 | 59 | 62 | 29,89 | 66 | | S | 2 | Cloudy. |
| 23 | 48 | 7 | 0 | 48 | 61 | 29,94 | 73 | 0,055 | SE | 1 | Fair. |
| | 60 | 2 | 0 | 60 | 62 | 29,87 | 61 | | SE | 1 | Fair. |
| 24 | 53 | 7 | 0 | 53 | 61 | 29,68 | 74 | | SSE | 1 | Fair. |
| | 55 | 2 | 0 | 54 | 63 | 29,74 | 54 | | NW | 1 | Fine. |
| 25 | 41 | 7 | 0 | 41 | 60 | 29,81 | 63 | 0,070 | E | 1 | Cloudy. |
| | 51 | 2 | 0 | 51 | 61 | 29,71 | 64 | | ESE | 1 | Rain. |
| 26 | 45 | 7 | 0 | 46 | 61 | 29,58 | 74 | 0,172 | S | 2 | Cloudy. |
| | 62 | 2 | 0 | 62 | 62 | 29,53 | 66 | | SSE | 2 | Cloudy. |
| 27 | 50 | 7 | 0 | 51 | 61 | 29,75 | 65 | | SE | 1 | Cloudy. |
| | 51 | 2 | 0 | 50 | 62 | 29,68 | 72 | | E | 1 | Rain. |
| 28 | 46 | 7 | 0 | 46 | 60 | 29,47 | 82 | 1,375 | N | 1 | Rain. |
| | 50 | 2 | 0 | 50 | 62 | 29,33 | 82 | | NE | 1 | Cloudy. |
| 29 | 36 | 7 | 0 | 36 | 59 | 29,50 | 77 | 0,240 | NE | 1 | Cloudy. |
| | 52 | 2 | 0 | 52 | 61 | 29,44 | 67 | | E | 2 | Cloudy. |
| 30 | 47 | 7 | 0 | 47 | 59 | 29,16 | 78 | 0,562 | W | 1 | Rain. |
| | 53 | 2 | 0 | 53 | 61 | 29,30 | 64 | | S | 2 | Cloudy. |
| 31 | 46 | 7 | 0 | 46 | 59 | 29,71 | 69 | 0,185 | SSW | 1 | Fine. |
| | 55 | 2 | 0 | 55 | 60 | 29,78 | 66 | | S | 1 | Cloudy. |

## METEOROLOGICAL JOURNAL

## for November, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| Nov. 1 | 48 | 7 | 0 | 53 | 60 | 29,69 | 77 | 0,060 | S | 2 | Rain. |
| | 60 | 2 | 0 | 60 | 62 | 29,57 | 61 | | SSE | 2 | Fair. |
| 2 | 54 | 7 | 0 | 54 | 61 | 29,37 | 73 | | S | 2 | Cloudy. |
| | 58 | 2 | 0 | 58 | 62 | 29,37 | 71 | | S | 2 | Rain. |
| 3 | 46 | 7 | 0 | 47 | 60 | 29,54 | 70 | 0,232 | S | 2 | Fine. |
| | 53 | 2 | 0 | 53 | 61 | 29,56 | 66 | | SSE | 2 | Cloudy. |
| 4 | 40 | 7 | 0 | 40 | 58 | 29,83 | 67 | 0,041 | S | 1 | Fine. |
| | 49 | 2 | 0 | 49 | 61 | 29,92 | 62 | | SSW | 1 | Fair. |
| 5 | 48 | 7 | 0 | 55 | 59 | 29,46 | 77 | 0,112 | S | 2 | Cloudy. |
| | 58 | 2 | 0 | 58 | 61 | 29,15 | 80 | | S | 2 | Rain. |
| 6 | 41 | 7 | 0 | 41 | 58 | 29,39 | 67 | 0,157 | SSW | 2 | Fine. |
| | 49 | 2 | 0 | 49 | 60 | 29,44 | 57 | | W | 1 | Fair. |
| 7 | 44 | 7 | 0 | 44 | 58 | 28,69 | 89 | 0,646 | SW | 1 | Rain. |
| | 47 | 2 | 0 | 47 | 59 | 28,83 | 74 | | NW | 2 | Cloudy. |
| 8 | 43 | 7 | 0 | 46 | 58 | 28,77 | 81 | 0,155 | S | 1 | Cloudy. |
| | 53 | 2 | 0 | 53 | 59 | 28,72 | 68 | | S | 1 | Fair. |
| 9 | 46 | 7 | 0 | 46 | 59 | 28,70 | 79 | 0,032 | WSW | 1 | Rain. |
| | 50 | 2 | 0 | 50 | 60 | 28,98 | 71 | | WNW | 1 | Rain. |
| 10 | 40 | 7 | 0 | 40 | 57 | 29,43 | 70 | 0,128 | W | 2 | Fine. |
| | 47 | 2 | 0 | 47 | 60 | 29,55 | 60 | | W | 1 | Fine. |
| 11 | 36 | 7 | 0 | 36 | 57 | 29,88 | 72 | 0,018 | SW | 1 | Fine. |
| | 45 | 2 | 0 | 45 | 59 | 29,98 | 63 | | SW | 1 | Fine. |
| 12 | 37 | 7 | 0 | 37 | 56 | 30,01 | 70 | | SSE | 1 | Fine. |
| | 45 | 2 | 0 | 45 | 59 | 29,96 | 64 | | SSE | 1 | Fine. |
| 13 | 37 | 7 | 0 | 37 | 56 | 29,90 | 70 | | ENE | 1 | Fair. |
| | 45 | 2 | 0 | 45 | 56 | 29,84 | 66 | | SE | 1 | Cloudy. |
| 14 | 36 | 7 | 0 | 36 | 55 | 29,68 | 74 | 0,107 | SSW | 1 | Fine. |
| | 47 | 2 | 0 | 47 | 56 | 29,67 | 63 | | SW | 1 | Cloudy. |
| 15 | 39 | 7 | 0 | 39 | 55 | 29,90 | 70 | 0,016 | N | 1 | Cloudy. |
| | 46 | 2 | 0 | 46 | 56 | 30,02 | 65 | | NE | 1 | Fine. |
| 16 | 33 | 7 | 0 | 34 | 54 | 30,09 | 69 | | NW | 1 | Fine. |
| | 43 | 2 | 0 | 43 | 56 | 30,14 | 65 | | N | 1 | Fine. |

## METEOROLOGICAL JOURNAL

## for November, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hy-gro-me-ter. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| Nov. 17 | 29 | 7 | 0 | 30 | 54 | 30,27 | 72 | | | | Foggy. |
| | 36 | 2 | 0 | 36 | 55 | 30,25 | 71 | | SW | 1 | Fair. |
| 18 | 25 | 7 | 0 | 25 | 52 | 30,15 | 73 | | | | Foggy. |
| | 35 | 2 | 0 | 35 | 54 | 30,07 | 66 | | SW | 1 | Fair. |
| 19 | 35 | 7 | 0 | 35 | 51 | 29,67 | 68 | | E | 1 | Cloudy. |
| | 38 | 2 | 0 | 38 | 52 | 29,53 | 80 | | E | 1 | Rain. |
| 20 | 33 | 7 | 0 | 33 | 51 | 29,85 | 74 | 0,420 | NE | 1 | Fine. |
| | 40 | 2 | 0 | 40 | 53 | 29,97 | 64 | | NE | 1 | Fine. |
| 21 | 29 | 7 | 0 | 29 | 50 | 29,91 | 73 | | NE | 1 | Fine. |
| | 35 | 2 | 0 | 34 | 52 | 29,75 | 65 | | E | 2 | Fine. |
| 22 | 31 | 7 | 0 | 32 | 48 | 29,35 | 78 | | E | 2 | Snow. |
| | 37 | 2 | 0 | 37 | 52 | 29,32 | 79 | | E | 2 | Rain. |
| 23 | 35 | 7 | 0 | 40 | 50 | 29,36 | 82 | 0,391 | E | 1 | Cloudy. |
| | 40 | 2 | 0 | 38 | 52 | 29,38 | 76 | | E | 1 | Cloudy. |
| 24 | 32 | 7 | 0 | 35 | 50 | 29,70 | 72 | | NE | 1 | Cloudy. |
| | 40 | 2 | 0 | 40 | 52 | 29,80 | 71 | | NE | 1 | Cloudy. |
| 25 | 25 | 7 | 0 | 27 | 50 | 29,88 | 75 | | W | 1 | Fine. |
| | 36 | 2 | 0 | 36 | 51 | 29,83 | 74 | | W | 1 | Cloudy. |
| 26 | 27 | 7 | 0 | 30 | 49 | 29,83 | 73 | | W | 1 | Fair. |
| | 42 | 2 | 0 | 41 | 52 | 29,65 | 77 | | SW | 1 | Cloudy. |
| 27 | 40 | 7 | 0 | 50 | 50 | 29,19 | 93 | 0,302 | S | 2 | Rain. |
| | 53 | 2 | 0 | 48 | 54 | 29,10 | 78 | | SSW | 2 | Rain. |
| 28 | 44 | 7 | 0 | 45 | 52 | 29,14 | 78 | 0,142 | SSW | 2 | Rain. |
| | 46 | 2 | 0 | 46 | 55 | 29,15 | 65 | | SSW | 2 | Fine. |
| 29 | 38 | 7 | 0 | 38 | 52 | 29,37 | 75 | 0,075 | SSW | 1 | Fair. |
| | 46 | 2 | 0 | 46 | 54 | 29,47 | 69 | | WSW | 1 | Fair. |
| 30 | 44 | 7 | 0 | 44 | 52 | 29,43 | 75 | 0,022 | SW | 1 | Cloudy. |
| | 46 | 2 | 0 | 46 | 54 | 29,44 | 67 | | W | 1 | Fair. |

## METEOROLOGICAL JOURNAL

## for December, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hy-gro-me-ter. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| | ° | | | | | | | | | | |
| Dec. 1 | 35 | 8 | 0 | 36 | 52 | 29,74 | 74 | | SW | 1 | Fair. |
| | 46 | 2 | 0 | 46 | 55 | 29,64 | 75 | | SSW | 1 | Rain. |
| 2 | 43 | 8 | 0 | 43 | 54 | 29,43 | 76 | 0,058 | SW | 1 | Cloudy. |
| | 46 | 2 | 0 | 46 | 56 | 29,42 | 67 | | SW | 1 | Cloudy. |
| 3 | 39 | 8 | 0 | 42 | 53 | 29,40 | 72 | | SSW | 2 | Cloudy. |
| | 47 | 2 | 0 | 47 | 56 | 29,48 | 67 | | W | 2 | Cloudy. |
| 4 | 34 | 8 | 0 | 35 | 54 | 29,93 | 75 | | W | 1 | Fair. |
| | 40 | 2 | 0 | 40 | 56 | 29,97 | 70 | | E | 1 | Fine. |
| 5 | 32 | 8 | 0 | 42 | 54 | 29,54 | 82 | 0,175 | E | 1 | Rain. |
| | 50 | 2 | 0 | 50 | 57 | 29,50 | 86 | | SE | 1 | Cloudy. |
| 6 | 47 | 8 | 0 | 47 | 55 | 29,53 | 90 | 0,256 | E | 2 | Rain. |
| | 48 | 2 | 0 | 47 | 57 | 29,66 | 85 | | E | 1 | Cloudy. |
| 7 | 42 | 8 | 0 | 42 | 55 | 29,90 | 83 | | E | 1 | Cloudy. |
| | 43 | 2 | 0 | 43 | 57 | 29,90 | 82 | | E | 1 | Cloudy. |
| 8 | 42 | 8 | 0 | 42 | 55 | 29,92 | 86 | | E | 1 | Foggy. |
| | 45 | 2 | 0 | 45 | 56 | 29,97 | 83 | | E | 1 | Cloudy. |
| 9 | 37 | 8 | 0 | 37 | 55 | 30,18 | 80 | | E | 1 | Fair. |
| | 40 | 2 | 0 | 40 | 56 | 30,23 | 79 | | ENE | 1 | Fine. |
| 10 | 34 | 8 | 0 | 34 | 54 | 30,26 | 68 | | ENE | 1 | Fine. |
| | 41 | 2 | 0 | 41 | 56 | 30,14 | 53 | | NE | 1 | Fine. |
| 11 | 31 | 8 | 0 | 32 | 51 | 30,05 | 62 | | E | 2 | Fine. |
| | 38 | 2 | 0 | 38 | 53 | 29,98 | 60 | | E | 2 | Fine. |
| 12 | 31 | 8 | 0 | 33 | 50 | 29,93 | 61 | | E | 2 | Fine. |
| | 36 | 2 | 0 | 36 | 52 | 29,80 | 60 | | E | 2 | Fair. |
| 13 | 29 | 8 | 0 | 30 | 48 | 29,54 | 65 | | E | 2 | Cloudy. |
| | 31 | 2 | 0 | 31 | 49 | 29,52 | 63 | | E | 2 | Cloudy. |
| 14 | 28 | 8 | 0 | 29 | 47 | 29,57 | 64 | | E | 2 | Cloudy. |
| | 32 | 2 | 0 | 32 | 48 | 29,58 | 64 | | E | 2 | Cloudy. |
| 15 | 30 | 8 | 0 | 34 | 47 | 29,41 | 84 | | | | Foggy. |
| | 43 | 2 | 0 | 42 | 50 | 29,40 | 87 | | E | 1 | Rain. |
| 16 | 41 | 8 | 0 | 42 | 48 | 29,49 | 93 | 0,137 | S | 1 | Cloudy. |
| | 46 | 2 | 0 | 45 | 51 | 29,51 | 93 | | S | 1 | Cloudy. |

## METEOROLOGICAL JOURNAL

## for December, 1798.

| 1798 | Six's Therm. least and greatest Heat. | Time. | | Therm. without. | Therm. within. | Barom. | Hygrometer. | Rain. | Winds. | | Weather. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ° | H. | M. | ° | ° | Inches. | | Inches. | Points. | Str. | |
| Dec. 17 | 44 | 8 | 0 | 45 | 50 | 29,36 | 95 | 0,147 | SSE | 1 | Cloudy. |
| | 48 | 2 | 0 | 48 | 53 | 29,27 | 93 | | SSE | 1 | Cloudy. |
| 18 | 45 | 8 | 0 | 45 | 52 | 29,36 | 90 | 0,016 | NW | 1 | Rain. |
| | 46 | 2 | 0 | 46 | 53 | 29,47 | 84 | | NNW | 1 | Cloudy. |
| 19 | 40 | 8 | 0 | 42 | 53 | 29,62 | 86 | 0,018 | NE | 1 | Rain. |
| | 42 | 2 | 0 | 41 | 54 | 29,80 | 77 | | NE | 1 | Cloudy. |
| 20 | 31 | 8 | 0 | 31 | 51 | 30,26 | 73 | 0,020 | NE | 1 | Fine. |
| | 36 | 2 | 0 | 36 | 53 | 30,35 | 70 | | NE | 1 | Fine. |
| 21 | 28 | 8 | 0 | 28 | 50 | 30,48 | 77 | | E | 1 | Foggy. |
| | 36 | 2 | 0 | 35 | 52 | 30,46 | 68 | | SE | 1 | Cloudy. |
| 22 | 32 | 8 | 0 | 35 | 50 | 30,22 | 68 | | S | 1 | Cloudy. |
| | 44 | 2 | 0 | 37 | 52 | 30,12 | 77 | | SW | 1 | Cloudy. |
| 23 | 37 | 8 | 0 | 37 | 51 | 30,26 | 77 | 0,030 | E | 1 | Cloudy. |
| | 39 | 2 | 0 | 39 | 51 | 30,33 | 68 | | ESE | 1 | Cloudy. |
| 24 | 28 | 8 | 0 | 28 | 48 | 30,53 | 65 | | E | 2 | Cloudy. |
| | 29 | 2 | 0 | 27 | 51 | 30,58 | 59 | | E | 2 | Fine. |
| 25 | 24 | 8 | 0 | 24 | 47 | 30,30 | 70 | | NE | 1 | Fine. |
| | 26 | 2 | 0 | 26 | 47 | 30,28 | 64 | | NE | 2 | Fair. |
| 26 | 20 | 8 | 0 | 21 | 45 | 30,08 | 70 | | NE | 1 | Fair. |
| | 22 | 2 | 0 | 22 | 45 | 30,01 | 66 | | NE | 1 | Fine. |
| 27 | 14 | 8 | 0 | 14 | 42 | 29,76 | 67 | | E | 1 | Snow. |
| | 18 | 2 | 0 | 17 | 43 | 29,59 | 67 | | E | 2 | Snow. |
| 28 | 15 | 8 | 0 | 15 | 40 | 29,54 | 66 | | NE | 1 | Cloudy. |
| | 17 | 2 | 0 | 17 | 43 | 29,74 | 62 | | N | 1 | Fair. |
| 29 | 11 | 8 | 0 | 16 | 38 | 30,12 | 69 | | SW | 1 | Fair. |
| | 32 | 2 | 0 | 32 | 42 | 30,18 | 72 | | SSW | 1 | Cloudy. |
| 30 | 32 | 8 | 0 | 32 | 40 | 30,51 | 87 | | E | 1 | Cloudy. |
| | 35 | 2 | 0 | 35 | 43 | 30,56 | 86 | | E | 1 | Cloudy. |
| 31 | 28 | 8 | 0 | 30 | 41 | 30,39 | 79 | | E | 1 | Cloudy. |
| | 34 | 2 | 0 | 32 | 43 | 30,34 | 80 | | E | 1 | Cloudy. |

| 1798. | Six's Therm. without. | | | Thermometer without. | | | Thermometer within. | | | Barometer.* | | | Hygrometer. | | | Rain. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Greatest height. | Least height. | Mean height. | Greatest height. | Least height. | Mean height. | Greatest height. | Least height. | Mean height. | Greatest height. | Least height. | Mean height. | Greatest height. | Least height. | Mean height. | |
| | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. | Deg. | Inches. | Inches. | Inches. | Deg. | Deg. | Deg. | Inches. |
| January | 53 | 28 | 39,6 | 53 | 29 | 40,1 | 59 | 49 | 53,5 | 30,52 | 28,96 | 29,94 | 90 | 73 | 82,8 | 1,105 |
| February | 54 | 24 | 39,9 | 54 | 24 | 40,1 | 58 | 49 | 54,2 | 30,76 | 29,23 | 30,11 | 90 | 71 | 82,2 | 0,693 |
| March | 58 | 30 | 42,9 | 58 | 30 | 42,9 | 61 | 50 | 55,4 | 30,37 | 29,18 | 29,93 | 90 | 68 | 79,8 | 0,333 |
| April | 69 | 30 | 51,6 | 69 | 31 | 52,7 | 66 | 53 | 59,8 | 30,38 | 29,27 | 29,96 | | | | 0,517 |
| May | 76 | 43 | 56,5 | 75 | 46 | 57,3 | 66 | 58 | 60,6 | 30,44 | 29,11 | 30,00 | 69 | 30 | 51,4 | 1,621 |
| June | 86 | 47 | 64 | 86 | 51 | 64,8 | 71 | 62 | 65,8 | 30,42 | 29,65 | 30,07 | 69 | 32 | 50,1 | 0,960 |
| July | 78 | 51 | 63,9 | 76 | 54 | 64,4 | 72 | 64 | 65,9 | 30,17 | 29,36 | 29,80 | 74 | 38 | 55,8 | 2,879 |
| August | 83 | 52 | 65,6 | 82 | 55 | 63,9 | 72 | 66 | 68,5 | 30,35 | 29,70 | 30,09 | 70 | 41 | | 1,525 |
| September | 76 | 44 | 58,9 | 76 | 45 | 59,2 | 70 | 58 | 64,2 | 30,26 | 28,97 | 29,78 | 73 | 37 | | 2,437 |
| October | 64 | 32 | 51,8 | 63 | 33 | 52,4 | 63 | 57 | 60,7 | 30,39 | 29,16 | 29,90 | 82 | 45 | | 3,428 |
| November | 60 | 25 | 41,6 | 60 | 25 | 42,4 | 62 | 48 | 55,4 | 30,27 | 28,69 | 29,58 | 93 | 57 | | 3,056 |
| December | 50 | 11 | 35,2 | 50 | 14 | 35,5 | 57 | 38 | 50,3 | 30,58 | 29,27 | 29,90 | 95 | 53 | | 0,857 |
| Whole year | | | 51,0 | | | 51,3 | | | 59,5 | | | 29,92 | | | | 19,411 |

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* The quicksilver in the bason of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

# PHILOSOPHICAL
# TRANSACTIONS,
OF THE
# ROYAL SOCIETY
OF
# LONDON.

FOR THE YEAR MDCCXCIX.

PART II.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCIX.

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# PHILOSOPHICAL TRANSACTIONS.

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## X. *An Account of the Dissection of an Hermaphrodite Dog. To which are prefixed, some Observations on Hermaphrodites in general.* *By* Everard Home, *Esq. F. R. S.*

Read March 7, 1799.

Instances of animals being brought forth, whose organs of generation are preternaturally formed, sometimes occur, and have been commonly called hermaphrodites; this term, however, should be confined to those only in which there is a mixture of the male and female organs in the same animal.

Examples of this kind have been rarely noticed; they have been met with at very distant periods of time, and confined to too few species of animals, to afford extensive opportunities for collecting observations respecting them. To this cause must be attributed, the little information that has been acquired upon so curious and interesting a subject.

Monstrous productions, having a mixture of the male and female organs, and which deserve the name of hermaphrodites, appear to arise most frequently in neat cattle; they are now generally known, and have been called free-martins.

This compound animal attracted the attention of the late Mr. HUNTER; and a Paper of his, containing a description of the organs of generation of different free-martins, to shew that they are by no means uniformly the same, or partake equally of the parts belonging to both sexes, is published in the Philosophical Transactions, Vol. LXIX. To add to these dissections an account of similar formation of the organs of generation in a dog, is the intention of the present Paper. The subject having already been considered an object deserving the attention of this learned Society, is an inducement for bringing forward new facts, and observations which have been made respecting them.

The causes of monstrous productions of every kind are at present equally unknown, but it is highly probable that they are very similar; and, when once they have been brought into action, it would be reasonable to suppose, that the influence should be continued to several young, in succession; this is, however, by no means the case, for, of all the monstrous productions that have come under my observation, none of them have been either immediately preceded, or followed, by a monster of the same, or of any other kind.

In the neat cattle, the free-martin is most commonly met with where there are twins; one is a free-martin, and the other is always a perfectly formed male. In the human species, there have been instances of mothers having alternately a perfect and a monstrous child; so that these observations lead to the idea, that monstrous productions do not follow immediately upon one another; that they sometimes alternate; but are commonly, as in the child with the double head, (an account of which has been laid before this Society,) only one in a family, of which the others are perfectly formed.

From Mr. Hunter's observations we learn, that in all the instances of free-martins which he examined, none had the complete organs of the male and female, but partly the one and partly the other, forming a mixture of both; and, what is deserving of notice, the ovaria and testicles, in all of them, were too imperfect to perform their functions.

There is much reason to believe, that no instance of an hermaphrodite, in the strict sense of the word, has ever occurred in the more perfect quadrupeds, or in the human species; for, when we consider the bones of the pelvis, to which the organs of generation are connected, it is difficult to conceive in what way the complete parts of the male and female could be placed, distinct from each other; and no instance of its having happened is to be found, in any record which can be depended upon.

As much has been said by authors, respecting hermaphrodites, particularly in our own species, and histories of them have a place even in the Philosophical Transactions, it may not be improper to explain the different kinds of monstrous production, which have been frequently mistaken for a complete mixture of male and female organs.

This inquiry into the subject of hermaphrodites, I shall pursue in the following order: first, examine into such malformations of the male, as led to the belief of the persons being hermaphrodites. Secondly, such malformations in the female, as have led to the same conclusion. Thirdly, such males as, from a deficiency in their organs, have not the character and general properties of the male, and may be called neuters. Fourthly, those in which there is a real mixture of the organs of both sexes, although not sufficiently complete to constitute double organs; which I believe to be the nearest approach towards an hermaphrodite that has been met with in the more perfect animals; and, it is extremely in favour of this opinion, that every account I have met with in authors, may be referred to one or the other of these heads.

Baron HALLER, who has laboured this subject with his usual perspicuity, has collected in one point of view, the histories of reputed hermaphrodites, from almost every author that preceded him;* and his conclusions are in confirmation of what is now advanced.

In considering the malformations of the male organs of generation, which put on the appearance of both the male and female organs, I cannot better illustrate the description, than by taking up the cases mentioned in CHESELDEN: one is a Negro, the other is an European.

From an examination of the engravings of that work,† no superficial observer would harbour a doubt of their being complete hermaphrodites; and the opinion of Dr. DOUGLAS, which is annexed, in favour of the existence of the female organs, strengthens CHESELDEN's authority. In these cases, however, there is much reason to believe, that the parts were entirely those belonging to the male, only very much distorted by an imperfection of the scrotum, which was divided into two separate bags, with a deep slit between them, resembling very much the labia pudendi, and the opening into the vagina; over these, hung down the penis: the imperfection of the septum of the scrotum extended to the canal of the urethra. This is not unlike the fissure in the hare-lip being continued through the bony palate, a circumstance often met with.

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* *Commentatio* A. HALLER, *de Hermaphroditis. Comment. Soc. Reg. Scient. Gòttingensis.* (Tom. I.)

† CHESELDEN's Anatomy of the Human Body, 8vo.

The under surface of the penis was attached, through its whole length, to the two bags containing the testicles, looking like a preternatural clitoris, to which it bore a more perfect resemblance, from the absence of the urethra.

The urine passed through a preternatural termination of the urethra in the perinæum, and came out externally, in the space between the testicles, which formed an enlarged aperture, that had been mistaken for a narrow vagina, in consequence of its allowing an instrument to pass to some distance, by conducting it to the bladder.

HALLER dissected a ram, in which the parts had been supposed to be those of an hermaphrodite. He found the animal to be a male, with the imperfections above mentioned; and, on comparing the dissection with many instances which have been stated by different writers, both in the human species and in quadrupeds, he considered them all to have been similar, in the conformation of the parts of generation.

Such malformation of the parts in the male, is particularly deserving of attention, as it is that which, more than any other, has been mistaken for a mixture of those of both sexes. It often occurs in different degrees of imperfection; and, in some instances, can be materially diminished by the assistance of the surgeon, although the greater number of cases are beyond the reach of art.

It may be supposed, that so great an imperfection in the structure of the penis, is necessarily attended with others in the more essential organs of generation; I shall therefore give an instance to the contrary. In a case of this kind, in which the canal was continued to the external orifice at the glans penis, the deficiency of the urethra behind the scrotum was so great, that every attempt to close the aperture necessarily left in perinæo proved ineffectual; and, under these circumstances, the person married. When he had connexion, the emission was complete, which proved that the testicles were perfect; but the semen always passed out at the perinæum.

The late Mr. HUNTER was consulted, to remedy, if possible, this inconvenience, and enable the person to beget children. After the failure of several modes of treatment which were adopted, Mr. HUNTER suggested the following experiment. He advised that the husband should be prepared with a syringe fitted for the purpose, previously warmed; and that, immediately after the emission had taken place, it should be taken up by the syringe, and injected into the vagina, while the female organs were still under the influence of the coitus, and in the proper state for receiving the semen.

This experiment was actually made, and the wife proved with child. Upon a subject of this kind it is proper to speak with caution; but, from all the attending circumstances, no doubt was entertained by Mr. HUNTER, or the husband, that the impregnation was entirely the effect of the experiment.

SPALLANZANI's experiments on this subject, upon animals, were made several years after this proposal of Mr. HUNTER's had been attended with success.

In the female, there are two malformations of the organs of generation, which give an appearance to the external parts, tending to mislead the judgment respecting the sex. One is, an enlargement of the clitoris; which is stated by authors to grow to an immoderate size in warm climates, and to resemble a penis. In cold climates, instances of this kind do not occur; and, even in hot countries, they are now rarely met with to such an extent. The accounts that have been given, we must suppose, are much exaggerated; for it is scarcely to be believed, that any enlargement which the clitoris is liable to, can give it a sufficient likeness to a penis, to be productive of any mistake.

The most remarkable instance of this kind, that has come to my knowledge, was a Negress, who was purchased by General MELVILLE, in the island of Dominica in the West Indies, about the year 1774. She was of the Mandingo nation, twenty-four years of age; her breasts were very flat; she had a rough voice, and masculine countenance. The clitoris was two inches long, and in thickness resembled a common sized thumb; when viewed at some distance, the end appeared round, and of a red colour, but, upon a closer examination, was found to be more pointed than that of a penis, not flat below, and having neither prepuce nor perforation; when handled, it became half erected, and was, in that state, fully three inches long, and much larger than before: when she voided her urine, she was obliged to lift it up, as it completely covered the orifice of the urethra. The other parts of the female organs were found to be in a natural state.

Dr. CLARK, who has favoured me with this account, from his own examination, mentions, that among the Negro women of the Mandingo and Ibbo nations, a large clitoris is very common; and, in several instances which came under his observation, in the course of his practice in midwifery, in the island of Dominica, the clitoris was an inch long, and thick in proportion; but attended with no other preternatural appearance.

The case above-mentioned, while it proves that the clitoris is sometimes of a very extraordinary size, also shews, that when so enlarged, it is unconnected with any mixture of the male organs.

The other malformation is a protrusion of the internal parts, which may be considered a prolapsus uteri, and therefore more a disease than an original malformation; it is probable, however, that if the parts had been perfectly formed, and acquired their due size, this change of their situation could not happen. The womb, thus displaced, has put on an appearance resembling a penis; and has been actually mistaken for one, even by medical men of character, who examined the parts.

The following case of this kind came under my own observation.

A French woman had a prolapsus uteri at an early age, which increased as she grew up; the cervix uteri was uncommonly narrow, and, at the time I saw her, (when she was about twenty-five years old,) projected several inches beyond the external opening of the vagina; the surface of the internal parts, from constant exposure, had lost its natural appearance, and resembled the external skin of the penis; the orifice of the os tincæ was mistaken for the orifice of the urethra. This woman was shewn as a curiosity in London; and, in the course of a few weeks, made four hundred pounds. I was induced by curiosity to visit her, and on the first inspection discovered the deception; which, although very complete to a common observer, must have been readily detected by any person intimately acquainted with anatomy. To render herself still more an object of curiosity, she pretended to have the powers of a male. As soon as the deception was found out, she was obliged to go away.

The history of an hermaphrodite is published in the XVIth Vol. of the Philosophical Transactions, which proves to be exactly similar to this, as is sufficiently ascertained by the menses flowing regularly through the orifice of the supposed penis. The French physicians were however so perfectly convinced of her manhood, that they made her change her dress, and learn a trade. To this she readily submitted; and the account says, she could perform very well the functions of a man, but not those of the other sex. This woman also was French.

It is probable, that the most common imperfection in the male organs of generation, is a defect in the structure of the testicle; that organ remaining in its fœtal state, and never becoming fitted to perform its functions. When this happens, the person cannot be considered of the masculine gender, but of the neuter; having, properly speaking, no sex. Such persons, in their general external form, have neither the true character of a man, nor that of a woman. These neuters are more common than is generally believed: they vary in their external appearance; some being an exact medium between the male and female, and others having a greater resemblance to the one or the other sex; which bias may be the result of turn of mind, occupation, or other circumstances. Probably, only those whose form is very like females, attract the notice of common observers, so as to have their defects discovered.

The following instances of children with male organs having remained neuters, in consequence of the testes being imperfectly formed, and incapable of producing that influence on the constitution which stamps it with the character of the sex, have come under my own observation.

A marine soldier, aged 23, in the year 1779, was admitted a patient into the Royal Naval Hospital at Plymouth, under my care. He had been there only a few days, when a suspicion arose of his being a woman, which induced me to examine into the circumstances. He proved to have no beard; his breasts were fully as large as those of a woman at that age; he was inclined to be corpulent; his skin uncommonly soft, for a man; his hands fat and short; his thighs and legs very much like those of a woman; the quantity of fat upon the os pubis, resembled the mons veneris; the penis was unusually small, as well as short, and not liable to erections; the testicles not larger in size than we commonly find them in the fœtal state; and he had never felt any passion for women.

In this case, the testicles had been imperfectly formed, and the constitution was deprived of that influence which it naturally receives from them. In addition to this imperfection in the organs of generation, he was weak in his intellects, and in his bodily strength.

The two following cases, shew a still greater degree of imperfection in the male organs.

A woman near Modbury, in Devonshire, the wife of a day labourer, had three children: the first was considered to be an hermaphrodite; the second was a perfectly formed girl; and the third an hermaphrodite, similar to the first. Having heard this account, I visited the cottage, in the year 1779, and made the following observations upon the imperfectly formed children.

The eldest was thirteen years of age, of a most uncommon bulk, which appeared to be almost wholly composed of fat; his body, round the waist, was equal to that of a fat man, and his thighs and legs in proportion; he was four feet high; his breasts as large as those of a fat woman; the mons veneris loaded with fat; no penis; a preputium one-sixth of an inch long; and under it the meatus urinarius, but no vagina. There was an imperfect scrotum, with a smooth surface, without a rapha in the middle, but, in its place, an indented line; it contained two testicles, of the size they are met with in the fœtus. He was very dull and heavy, almost an idiot, but could walk and talk: he began to walk at a year and a half old.

The younger one was six years old, uncommonly fat, and large for his age; more an idiot than the other, not having sense enough to learn to walk, although his limbs were not defective. The external parts of generation differed in nothing from those just described, except in the prepuce being an inch long. He had a supernumerary finger on each hand, and a supernumerary toe on each foot.

It is curious that the mother of these two children, so like in their imperfections, should have had a perfectly formed child between them; and it leads me to mention, that the Polish dwarf, Count Boruwlaski, who was in England in 1786, stated, that in his family the children had been alternately dwarfs, of which there were two, him and his sister; the intermediate child having grown to the common size.

The immense accumulation of fat, and the uncommon size of these children, accords with the disposition to become fat, so commonly met with in the free-martin.

The species of malformation of the organs of generation in which there is really a mixture of parts, or an evident attempt towards it, is less common than those we have mentioned. Mr. Hunter has given several instances of it in the neat cattle, where the mixture of male and female organs was in different degrees. In two free-martins, imperfect testicles were found in the situation of the ovaria; and, in a third, an appearance like both testicles and ovaria was met with, close together, in the situation of the ovaria. He also gives the dissection of an hermaphrodite ass; in which there were substances resembling both testicles and ovaria in the abdomen.

Mr. HUNTER never met with an instance of this kind in the dog; and I have not found one in any record which I have examined. I shall therefore state the following history of a case which has fallen under my own observation, as it proves, that a mixture of the generative organs sometimes occurs, in a species of animal in which it had not been before met with; and, as the dog is more domesticated than almost any other quadruped, the occurrence must be rare indeed, otherwise it could not have escaped notice.

A favourite dog of Lord BESSBOROUGH'S, which had lived in the family for many years, was observed to have no teats, and never to have been in heat, although, to appearance, a perfectly formed bitch in all other respects: those circumstances being made known to Sir JOSEPH BANKS, he requested, that when the animal died, it might be sent to him. This happened last summer; and I had an opportunity of examining the organs of generation, which exhibited the following appearances. As they are represented in the annexed drawing, (Tab. IV.) it will be less necessary to be very minute in my description.

There was not the smallest appearance of teats on the skin of the belly; so that, in this particular, it differed both from the male and female; nor was there the least trace of any thing like the gland of the breast, under the skin. The clitoris was very large, being three-quarters of an inch long, and half an inch broad; the orifice of the meatus urinarius was unusually large, as if it was intended for a common passage to the bladder and vagina; so that the external parts were only the clitoris, meatus urinarius, and rectum. Internally, in the situation of the ovaria, were two imperfectly formed small testicles, distinguished to be such by the convolutions of the spermatic artery; from these passed down an impervious chord, or vas deferens, not thicker than a thread, to the posterior part of the bladder, where they united into one substance, which was nearly two inches long, and terminated behind the meatus urinarius. The other parts of the animal were naturally formed. When the testicles were cut into, they appeared to have no regular glandular structure.

In this animal, the clitoris was the only part of the female organs that was completely formed. What rendered the parts a decided mixture of male and female organs was, the testes being in the place appropriated for the ovaria, and the ligamentous substance, to which the vasa deferentia were connected, somewhat resembling an impervious vagina. The clitoris, in this instance, could not be considered as an imperfect penis, since the bone, the distinguishing mark of the dog's penis, was wanting.

In Haller's account of hermaphrodites, before mentioned, there is the history of a kid, in which there was a mixture of male and female organs, illustrated by an engraving. They were very similar to those of this dog: the imperfect testicles were in the same situation; but there was a pervious canal or vagina, that divided, like the uterus, into two horns, which extended to the testicles; there were also vesiculæ seminales. In the Memoirs of the Royal Academy of Sciences of Paris,* there is a very accurate description, by M. PETIT, of a similar mixture of organs in the human species. The person had wholly the character of a man, but was of a delicate constitution: he was a soldier, and died of his wounds. The appearance of the penis is passed over; but the scrotum, not containing testicles, drew M. PETIT's attention; and, in the dissection, he found testicles in the situation of the ovaria, attached to two processes, continued from an imperfect vagina, but having vasa deferentia, which passed, in the usual manner, to the vesiculæ seminales. The vagina communicated with the urethra, between the neck of the bladder and the prostate gland.

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* For the year 1720.

A case of mixed organs is mentioned in Dr. BAILLIE's Morbid Anatomy: the person was twenty-four years of age, had the breasts of a woman, and no beard. The clitoris and meatus urinarius had the natural appearance, but there were no nymphæ, and the labia pudendi were unusually pendulous, containing a testicle in each of them. The vagina was nearly two inches long, and terminated in a blind end. She never menstruated, and had a masculine appearance.

This appears to have been the reverse of the case mentioned by M. PETIT: in this, the external parts were those of the female, in which were contained the testicles; while, in the other, the internal parts were those of the female, with the testicles attached to them.

There is still another mixture of the organs of the female with those of the male, which is probably the most rare in its occurrence; this is, an hermaphrodite bull, probably a free-martin, partaking so much of the bull as to have the male organs capable of propagating the species, and an udder capable of secreting milk.

The glands which secrete milk, although in themselves not organs of generation, entirely belong to them, and form a part of the female character, sufficiently obvious to connect them intimately with the present subject.

That an animal not a perfect female, should have glands which secrete milk, or indeed that an animal truly female, without having had young, should give milk, is so extraordinary, that even written evidence respecting it requires confirmation to entitle it to credit; in this respect, the following fact must be considered as perfectly satisfactory.

An instance of an hermaphrodite bull, whose udder secreted milk, occurred lately in Poland. The animal came into the possession of Mr. Brookes, who procured it near Grodno, in the year 1796, and carried it to St. Petersburg, where it died in the following year; unfortunately, no examination was made after death.

While the animal was at St. Petersburg, both Dr. Rogerson and Dr. Rogers had opportunities of examining it with a considerable degree of accuracy; and the following account is taken from their description, with which I have been favoured by Dr. Rogerson, who is now in London.

The animal was under the usual size of neat cattle, and is stated by Mr. Brookes to have been about fifteen years old; it was in a weakly state, and Mr. Brookes told Dr. Rogerson, that he had much difficulty in making it bear the journey from Grodno, (a distance of about 800 miles) and was obliged to give it the most nourishing diet, which was principally ground malt.

In its general appearance, the male character predominated, particularly in the head, horns, neck, shoulders, and organs of generation. The flanks and hind quarters had a greater resemblance to those of the cow.

The penis was of the ordinary size, and had the common appearance; the preputium had the tuft of hair at the orifice, as in the bull. The urine was ejected through the penis.

It had an udder in the common situation, which was smaller, and more globular, than that of the cow, and its teats were less pendulous. Dr. ROGERS found one of the testicles, by pressing upon the udder; but was unable to detect the other.

There was an external orifice in the situation of the vagina, but so small as not to appear capable of receiving more than the point of the fore finger. Dr. ROGERSON thinks, from its appearance, that it never could have admitted the male, much less have brought forth a calf, which had been asserted, but without any proof whatever. Mr. BROOKES, who is now in this country, admits that it had never received the male, or brought forth young, while in his possession; but asserts that it had several times covered the female, and had begot five calves. This assertion, Dr. ROGERSON thinks highly deserving of credit.

The udder contained milk capable of giving cream, but the quantity was very small. When Dr. ROGERSON was present, only an ounce could be procured; but he was told that at other times a tea-cupful was drawn. Mr. BROOKES states, that he once saw an English pint milked at one time.

As the teats of the bull are in the same situation as those of the cow, it became an object of inquiry, whether any males of that tribe of animals, that were not hermaphrodites, had ever been known to give milk; and I find there are two instances recorded in the Philosophical Transactions, of wethers having given suck.

One is upon the testimony of the Rev. Dr. DODDRIDGE, who states that a lamb was nourished by the milk, and, when the teats were pressed, milk came out.*

The other is on the testimony of Mr. KIRKE, of Cookridge, in Yorkshire. He mentions, that Sir WILLIAM LOWTHER had a lamb suckled by a wether. The lamb sucked during the whole summer, and, after it was weaned, milk could be pressed from the teats: each side of the udder was the size of an hen's egg. This account is dated Sept. 28th, 1694. He gives a second relation, in November, stating that the udder was reduced in size, but there was still some milk in it, and no appearance of the animal being an hermaphrodite.†

A case is also recorded in the Philosophical Transactions, of a man giving suck to a child two months old;‡ this, however, is not stated with sufficient accuracy to allow any stress being laid upon it, although it would have been improper not to have noticed it in this place.

In considering the influence of the testicles upon the constitution of the male, which is rendered so evident by contrasting it with those cases in which the testicles are imperfect, it leads to a supposition, that the ovaria may have a similar influence upon the constitution of the female; and that, when the ovaria are imperfectly formed, or when testicles are substituted for them, although the external parts are decidedly female, the person may grow up, deprived of that feminine character which the constitution would have acquired, if the ovaria had been capable of producing their influence on the body. To this cause may be attributed the unnatural bias which some women have shewn, to pass through life in the character of men. The circumstance of some women, after the time of breeding is over, (at which period the influence of the ovaria may be considered as lost to the constitution,) approaching nearer to the male in appearance, and acquiring a beard; also the female pheasant and duck,* in several instances, at the same period of their life, acquiring the feathers which distinguish the male, so as to be mistaken for males, is in favour of such an opinion.

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* Phil. Trans. Vol. XLV. p. 502.

† Phil. Trans. Vol. XVIII. p. 263.

‡ Phil. Trans. Vol. XLI. p. 813.

The histories of monsters which have superfluous parts, as that of the child with the double head,† and all others of the same kind, lead to the opinion of two or more fœtuses having been contained in one ovum, similar to two yolks in one egg; and that, from some circumstance having taken place in utero, certain parts of one of the fœtuses were prevented from coming to perfection, and were absorbed; while those that remained became connected to the other fœtus.

* The following account of a duck of this kind was sent me by Mr. RUMBALL, surgeon, at Abingdon, in Berkshire.

The duck was bred by Mr. CATOR, of Norwood, in Surrey, in the year 1781. It continued to lay, and to hatch its young, till the year 1789; when the curled feathers, peculiar to the drake, made their appearance in its tail. From this period, she not only left off laying, but frequently attempted to tread the other ducks, both in the water, and upon the ground; and they courted her in return. This was particularly observed on the 19th of August, 1791, when she trod a duck in the water, and fell off on her side, as drakes usually do; and they both began washing themselves immediately after, as is customary on these occasions. She never afterwards suffered a drake to come near her.

Although the plumage changed, the voice continued the same, which is very different from that of the drake. This circumstance first attracted Mr RUMBALL's notice, and made him doubt of its being really a drake. On the 14th of October, 1793, at the request of Mr. RUMBALL, this duck was sent to Mr. HUNTER, and died on the 18th, two days after Mr. HUNTER's death. On examination, the organs of generation were those of a perfect duck. The skin is stuffed, and preserved in Mr. HUNTER's collection.

† Phil. Trans. Vol. LXXX. p. 296, and this Vol. p. 28.

Where monsters are imperfect, there is no difficulty in accounting for any organ, or other structure, not having been completely formed; but, that the ovaria should be wanting, and their place supplied by testicles, is not to be explained upon the same principle.

The testicles being substituted for the ovaria, and the ovaria themselves entirely wanting, is probably the most curious circumstance that is met with, in the structure of these hermaphrodites; and, as many important discoveries in the animal œconomy have been suggested from the examination of monstrous productions, it naturally leads to the inquiry, whether there is any thing in the original formation of the parts, which can account for so strange an occurrence.

The only mode in which it can be explained, as far as I am able to judge, appears to be the following. By supposing the ovum, previous to impregnation, to have no distinction of sex, but to be so formed as to be equally fitted to become a male or female fœtus; and that it is the process of impregnation which marks the distinction, and conduces to produce either testicles or ovaria, out of the same materials.

The following circumstances are in favour of this opinion. The testicles and ovaria are formed originally in the same situation, although the testicles, even before the fœtus has advanced to the eighth month, are to change their situation, to a part at a considerable distance.

The clitoris, in fœtuses under four months, is so large as to be often mistaken for a penis. Preparations to show the size of the clitoris at this age are preserved in Mr. Hunter's collection; and M. Ferrien mentions it, with a view to explain an erroneous opinion which prevailed in France,* that the greater number of miscarriages between three and four months, have been remarked to be males; which mistake arose from the above circumstance.

The clitoris, originally, appears therefore equally fitted to be a clitoris or penis, as it may be influenced by the ovarium or testicle.

In considering this subject, it is curious to observe the number of secondary parts, which appear so contrived that they may be equally adapted to the organs of the male or female.

In those quadrupeds whose females have mammæ inguinales, the males have also teats in the same situation; so that the same bag which contains the testicles of the male, is adapted to the mammæ of the female.

In the human species, which have the mammæ pectorales, the scrotum of the male serves the purpose of forming the labia pudendi of the female, and the preputium makes the nymphæ. The male has pectoral nipples, as well as the female; and, in many infants, milk, or a fluid analogous to it, is secreted; which proves the existence of a glandular structure under the nipple.

This circumstance, when added to the instances already related, of an hermaphrodite bull, and of wethers giving suck, affords a strong presumption that the rudiments of the mammæ exist in the male, and, in some few instances, have been brought to perfection, either by an original mixture of organs, early emasculation, or other changes with which we are at present unacquainted.

If it is allowed that the sex is impressed upon the ovum at the time of impregnation, it may in some measure account for the free-martin occurring when two young are to be impressed with different sexes, at one impregnation; which must be a less simple operation, and therefore more liable to a partial failure, than when two or any greater number of ova are impressed with the same sex. It may also account for twins being most commonly of the same sex; and, when they are of different sexes, it leads us to inquire whether the female, when grown up, has not in some instances less of the true female character than other women, and is not incapable of having children. It is curious, and in some measure to the purpose, that in some countries, nurses and midwives have a prejudice, that such twins seldom breed.

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* *Mém. de l'Acad. Royal des Sciences de Paris,* 1767, p. 330.

This view of the subject throws some light on those cases where the testicles are substituted for the ovaria; since, whenever the impregnation fails in stamping the ovum with a perfect impression of either sex, the part formed will neither be an ovarium nor a testicle, sometimes bearing a greater resemblance to the one, sometimes to the other; and may, according to circumstances, either remain in the natural situation of the ovaria, or pass into the situation proper to the testicle, whether it is the scrotum of the male, or the labia pudendi of the female.

## EXPLANATION OF THE PLATE. (Tab. IV.)

The plate consists of two figures, which represent the anterior and posterior view of the organs of generation, in the hermaphrodite dog.

Fig. 1. The anterior view.

*a* The clitoris.

*b* The meatus urinarius.

*c* The bladder.

*dd* The testicles, and convolutions of the spermatic artery.

*ee* The impervious vasa deferentia.

Fig. 2. The posterior view.

*aa* The testicles.

*bb* The impervious vasa deferentia.

*c* The bladder.

*d* The union of the vasa deferentia, forming a substance which may be considered as an impervious vagina.

*e* The termination of the vasa deferentia at the neck of the bladder.

*Fig. 1.*

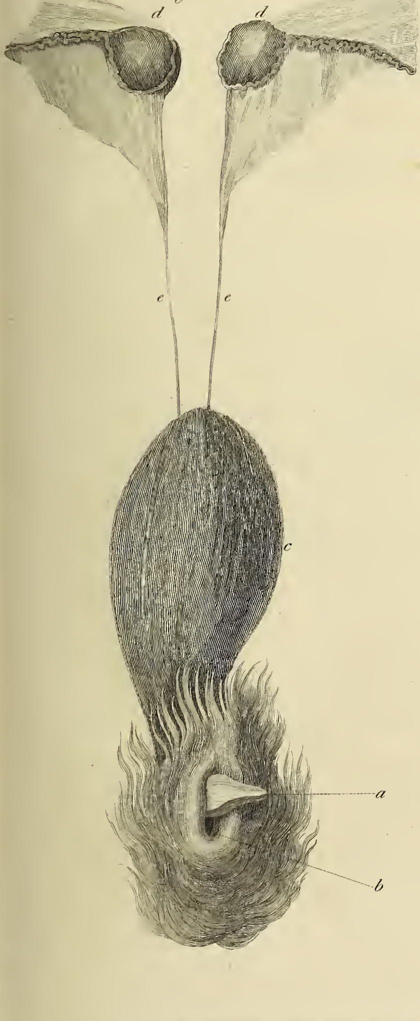
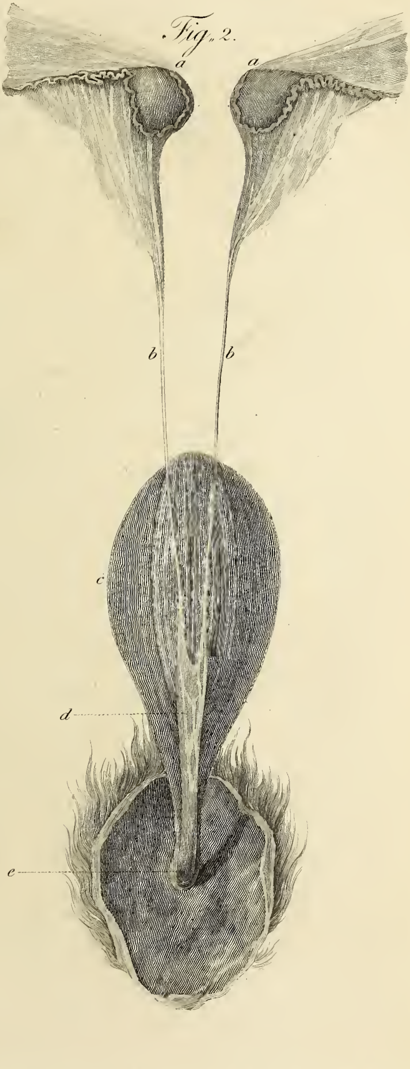


*Fig. 2.*



BRITISH MUSEUM NATURAL HISTORY.

# XI. *An Inquiry concerning the Weight ascribed to Heat.* *By* Benjamin *Count of* Rumford, *F. R. S. M. R. I. A. &c.*

Read May 2, 1799.

The various experiments which have hitherto been made with a view to determine the question so long agitated, relative to the weight which has been supposed to be gained, or to be lost, by bodies upon their being heated, are of a nature so very delicate, and are liable to so many errors, not only on account of the imperfections of the instruments made use of, but also, of those, much more difficult to appreciate, arising from the vertical currents in the atmosphere, caused by the hot or the cold body which is placed in the balance, that it is not at all surprising that opinions have been so much divided, relative to a fact so very difficult to ascertain.

It is a considerable time since I first began to meditate on this subject, and I have made many experiments with a view to its investigation; and in these experiments, I have taken all those precautions to avoid errors, which a knowledge of the various sources of them, and an earnest desire to determine a fact which I conceived to be of importance to be known, could inspire; but, though all my researches tended to convince me more and more, that *a body acquires no additional weight upon being heated,* or rather, that heat has no effect whatever upon the weights of bodies, I have been so sensible of the delicacy of the inquiry, that I was for a long time afraid to form a decided opinion upon the subject.

Being much struck with the experiments recorded in the Transactions of the Royal Society, Vol. LXXV. made by Dr. Fordyce, upon the weight said to be acquired by water upon being frozen; and being possessed of an excellent balance, belonging to *His most Serene Highness the* Elector Palatine Duke *of* Bavaria; early in the beginning of the winter of the year 1787,—as soon as the cold was sufficiently intense for my purpose,—I set about to repeat those experiments, in order to convince myself whether the very extraordinary fact related, might be depended on; and, with a view to removing, as far as was in my power, every source of error and deception, I proceeded in the following manner.

Having provided a number of glass bottles, of the form and size of what in England is called a Florence flask,—blown as thin as possible,—and of the same shape and dimensions, I chose out from amongst them two, which, after using every method I could imagine of comparing them together, appeared to be so much alike as hardly to be distinguished.

Into one of these bottles, which I shall call A, I put 4107,86 grains Troy of pure distilled water, which filled it about half full; and into the other, B, I put an equal weight of weak spirit of wine; and, sealing both the bottles hermetically, and washing them, and wiping them perfectly clean and dry on the outside, I suspended them to the arms of the balance, and placed the balance in a large room, which for some weeks had been regularly heated every day by a German stove, and in which the air was kept up to the temperature of 61° of Fahrenheit's thermometer, with very little variation. Having suffered the bottles, with their contents, to remain in this situation till I conceived they must have acquired the temperature of the circumambient air, I wiped them afresh, with a very clean dry cambric handkerchief, and brought them into the most exact equilibrium possible, by attaching a small piece of very fine silver wire to the arm of the balance to which the bottle which was the lightest was suspended.

Having suffered the apparatus to remain in this situation about twelve hours longer, and finding no alteration in the relative weights of the bottles,—they continuing all this time to be in the most perfect equilibrium,—I now removed them into a large uninhabited room, fronting the north, in which the air, which was very quiet, was at the temperature of 29°, F; the air without doors being at the same time at 27°; and, going out of the room, and locking the door after me, I suffered the bottles to remain forty-eight hours, undisturbed, in this cold situation, attached to the arms of the balance as before.

At the expiration of that time, I entered the room,—using the utmost caution not to disturb the balance,—when, to my great surprise, I found that the bottle A very sensibly preponderated.

The water which this bottle contained was completely frozen into one solid body of ice; but the spirit of wine, in the bottle B, showed no signs of freezing.

I now very cautiously restored the equilibrium, by adding small pieces of the very fine wire of which gold lace is made, to the arm of the balance to which the bottle B was suspended, when I found that the bottle A had augmented its weight by $\frac{1}{35904}$ part of its whole weight at the beginning of the experiment; the weight of the bottle with its contents having been 4811,23 grains Troy, (the bottle weighing 703,37 grains, and the water

4107,86 grains,) and it requiring now $\frac{134}{1000}$ parts of a grain, added to the opposite arm of the balance, to counterbalance it.

Having had occasion just at this time to write to my friend, Sir CHARLES BLAGDEN, upon another subject, I added a postscript to my letter, giving him a short account of this experiment, and telling him how "*very contrary to my expectation*" the result of it had turned out; but I soon after found that I had been too hasty in my communication. Sir CHARLES, in his answer to my letter, expressed doubts respecting the fact; but, before his letter had reached me, I had learned from my own experience, how very dangerous it is, in philosophical investigations, to draw conclusions from single experiments.

Having removed the balance, with the two bottles attached to it, from the cold into the warm room, (which still remained at the temperature of 61°), the ice in the bottle A gradually thawed; and, being at length totally reduced to water, and this water having acquired the temperature of the surrounding air, the two bottles, after being wiped perfectly clean and dry, were found to weigh as at the beginning of the experiment, before the water was frozen.

This experiment being repeated, gave nearly the same result, the water appearing, when frozen, to be heavier than in its fluid state; but, some irregularity in the manner in which the water lost the additional weight which it had appeared to acquire upon being frozen, when it was afterwards thawed, as also a sensible difference in the quantities of weight apparently acquired in the different experiments, led me to suspect, that the experiment could not be depended on for deciding the fact in question; I therefore set about to repeat it, with some variations and improvements; but, before I give an account of my further investigations relative to this subject, it may not be amiss to mention the method I pursued for discovering whether the appearances mentioned in the foregoing experiments might not arise from the imperfections of my balance; and it may likewise be proper to give an account, in this place, of an intermediate experiment which I made, with a view to discover, by a shorter route, and in a manner less exceptionable than that above mentioned, whether bodies actually lose, or acquire, any weight, upon acquiring an additional quantity of latent heat.

My suspicions respecting the accuracy of the balance arose from a knowledge,—which I acquired from the maker of it,—of the manner in which it was constructed.

The three principal points of the balance having been determined, as nearly as possible, by measurement, the axes of motion were firmly fixed in their places, in a right line, and the beam being afterwards finished, and its two arms brought to be in equilibrio, the balance was proved by suspending weights, which before were known to be exactly equal, to the ends of its arms.

If with these weights the balance remained in equilibrio, it was considered as a proof that the beam was just; but, if one arm was found to preponderate, the other was gradually lengthened, by beating it upon an anvil, until the difference of the lengths of the arms was reduced to nothing, or until equal weights, suspended to the two arms, remained in equilibrio; care being taken, before each trial, to bring the two ends of the beam to be in equilibrio, by reducing, with the file, the arm which had been lengthened.

Though, in this method of constructing balances, the most perfect equality in the lengths of the arms may be obtained, and consequently the greatest possible accuracy, when used at a time when the temperature of the air is the same as when the balance was made, yet, as it may happen, that in order to bring the arms of the balance to be of the same length, one of them may be much more hammered than the other, I suspected it might be possible that the texture of the metal forming the two arms might be rendered so far different, by this operation, as to occasion a difference in their expansions with heat; and that this difference might occasion a sensible error in the balance, when, being charged with a great weight, it should be exposed to a considerable change of temperature.

To determine whether the apparent augmentation of weight, in the experiments above related, arose in any degree from this cause, I had only to repeat the experiment, causing the two bottles A and B to change places upon the arms of the balance; but, as I had already found a sensible difference in the results of different repetitions of the same experiment, made as nearly as possible under the same circumstances, and as it was above all things of importance to ascertain the accuracy of my balance, I preferred making a particular experiment for that purpose.

My first idea was, to suspend to the arms of the balance, by very fine wires, two equal globes of glass, filled with mercury, and, suffering them to remain in my room till they should have acquired the known temperature of the air in it, to have removed them afterward into the cold, and to have seen if they still remained in equilibrio, under such difference of temperature; but, considering the obstinacy with which moisture adheres to the surface of glass, and being afraid that, somehow or other, notwithstanding all my precautions, one of the globes might acquire or retain more of it than the other, and that by that means its apparent weight might be increased; and having found by a former experiment, of which I have already had the honour of communicating an account to the Royal Society, that the gilt surfaces of metals do not attract moisture; instead of the glass globes filled with mercury, I made use of two equal solid globes of brass, well gilt and burnished, which I suspended to the arms of the balance, by fine gold wires.

These globes, which weighed 4975 grains each, being wiped perfectly clean, and having acquired the temperature (61°) of my room, in which they were exposed more than twenty-four hours, were brought into the most scrupulous equilibrium, and were then removed, attached to the arms of the balance, into a room in which the air was at the temperature of 26°, where they were left all night.

The result of this trial furnished the most satisfactory proof of the accuracy of the balance; for, upon entering the room, I found the equilibrium as perfect as at the beginning of the experiment.

Having thus removed my doubts respecting the accuracy of my balance, I now resumed my investigations relative to the augmentation of weight which fluids have been said to acquire upon being congealed.

In the experiments which I had made, I had, as I then imagined, guarded as much as possible against every source of error and deception. The bottles being of the same size, neither any occasional alteration in the pressure of the atmosphere during the experiment, nor the necessary and unavoidable difference in the densities of the air in the hot and in the cold rooms in which they were weighed, could affect their apparent weights; and their shapes and their quantities of surface being the same, and as they remained for such a considerable length of time in the heat and cold to which they were exposed, I flattered myself that the quantities of moisture remaining attached to their surfaces, could not be so different as sensibly to effect the results of the experiments.—But, in regard to this last circumstance, I afterwards found reason to conclude that my opinion was erroneous.

Admitting the fact stated by Dr. FORDYCE,—(and which my experiments had hitherto rather tended to corroborate than to contradict,)—I could not conceive any other cause for the augmentation of the apparent weight of water, upon its being frozen, than the loss of so great a proportion of its latent heat as that fluid is known to evolve when it congeals; and I concluded, that if the loss of latent heat added to the weight of one body, it must of necessity produce the same effect on another, and consequently, that the augmentation of the quantity of latent heat must,—in all bodies,—and in all cases,—diminish their apparent weights.

To determine whether this is actually the case or not, I made the following experiment.

Having provided two bottles, as nearly alike as possible, and in all respects similar to those made use of in the experiments abovementioned, into one of them I put 4012,46 grains of water, and into the other an equal weight of mercury; and, sealing them hermetically, and suspending them to the arms of the balance, I suffered them to acquire the temperature of my room, 61°; then, bringing them into a perfect equilibrium with each other, I removed them into a room in which the air was at the temperature of 34°, where they remained twenty-four hours.—But there was not the least appearance of either of them acquiring, or losing, any weight.

Here it is very certain, that the quantity of heat lost by the water, must have been very considerably greater than that lost by the mercury; the specific quantities of latent heat in water and in mercury, having been determined to be to each other as 1000 to 33; but this difference in the quantities of heat lost, produced no sensible difference on the weights of the fluids in question.

Had any difference of weight really existed, had it been no more than *one millionth* part of the weight of either of the fluids, I should certainly have discovered it;—and, had it amounted to so much as $\frac{1}{7000000}$ part of that weight, I should have been able to have measured it; so sensible, and so very accurate, is the balance which I used in these experiments.

I was now much confirmed in my suspicions, that the apparent augmentation of the weight of the water upon its being frozen, in the experiments before related, arose from some accidental cause; but I was not able to conceive what that cause could possibly be,—unless it were, either a greater quantity of moisture attached to the external surface of the bottle which contained the water, than to the surface of that containing the spirits of wine,—or some vertical current or currents of air, caused by the bottles or one of them not being exactly of the temperature of the surrounding atmosphere.

Though I had foreseen, and, as I thought, guarded sufficiently against, these accidents,—by making use of bottles of the same size and form,—and which were blown of the same kind of glass, —and at the same time,—and by suffering the bottles, in the experiments, to remain for so considerable a length of time exposed to the different degrees of heat and of cold, which alternately they were made to acquire; yet, as I did not know the relative conducting powers of ice and of spirit of wine with respect to heat; or, in other words,—the degrees of facility or difficulty with which they acquire the temperature of the medium in which they are exposed;—or the time taken up in that operation; and, consequently, was not *absolutely certain* as to the equality of the temperatures of the contents of the bottles at the time when their weights were compared, I determined now to repeat the experiments, with such variations as should put the matter in question out of all doubt.

I was the more anxious to assure myself of the real temperatures of the bottles and of their contents, as any difference in their temperatures might vitiate the experiment, not only by causing unequal currents in the air, but also, by causing, at the same time, a greater or less quantity of moisture to remain attached to the glass.

To remedy these evils, and also to render the experiment more striking and satisfactory in other respects, I proceeded in the following manner.

Having provided three bottles, A, B, and C, as nearly alike as possible, and resembling in all respects those already described; into the first, A, I put 4214,28 grains of water, and a small thermometer, made on purpose for the experiment, and suspended in the bottle in such a manner that its bulb remained in the middle of the mass of water; into the second bottle, B, I put a like weight of spirit of wine, with a like thermometer; and, into the bottle C, I put an equal weight of mercury.

These bottles, being all hermetically sealed, were placed in a large room,—in a corner far removed from the doors and windows, and where the air appeared to be perfectly quiet; and, being suffered to remain in this situation more than twenty-four hours, the heat of the room (61°) being kept up all that time with as little variation as possible, and the contents of the bottles A and B appearing, by their inclosed thermometers, to be exactly at the same temperature, the bottles were all wiped with a very clean dry cambric handkerchief; and, being afterwards suffered to remain exposed to the free air of the room a couple of hours longer, in order that any inequalities in the quantities of heat,—or of the moisture attached to their surfaces,—which might have been occasioned by the wiping, might be corrected by the operation of the atmosphere by which they were surrounded, they were all weighed, and were brought into the most exact equilibrium with each other, by means of small pieces of very fine silver wire, attached to the necks of those of the bottles which were the lightest.

This being done, the bottles were all removed into a room in which the air was at 30°, where they were suffered to remain, perfectly at rest and undisturbed, forty-eight hours; the bottles A and B being suspended to the arms of the balance, and the bottle C suspended, at an equal height, to the arm of a stand constructed for that purpose, and placed as near the balance as possible, and a very sensible thermometer suspended by the side of it.

At the end of forty-eight hours,—during which time the apparatus was left in this situation,—I entered the room, opening the door very gently, for fear of disturbing the balance; when I had the pleasure to find the three thermometers,—*viz.* that in the bottle A, (which was now inclosed in a solid cake of ice,)—that in the bottle B,—and that suspended in the open air of the room, all standing at the same point, 29° F, and the bottles A and B *remaining in the most perfect equilibrium.*

To assure myself that the play of the balance was free, I now approached it very gently, and caused it to vibrate; and I had the satisfaction to find,—not only that it moved with the utmost freedom,—but also,—when its vibration ceased,—that it rested precisely at the point from which it had set out.

I now removed the bottle B from the balance, and put the bottle C in its place; and I found that *that* likewise remained of the same apparent weight as at the beginning of the experiment, being in the same perfect equilibrium with the bottle A as at first.

I afterwards removed the whole apparatus into a warm room, and, causing the ice in the bottle A to thaw, and suffering the three bottles to remain till they and their contents had acquired the exact temperature of the surrounding air, I wiped them very clean, and, comparing them together, I found their weights remained unaltered.

This experiment I afterwards repeated several times, and always with precisely the same result; the water, *in no instance*, appearing to gain, or to lose, the least weight, upon being frozen, or upon being thawed; neither were the relative weights of the fluids in either of the other bottles in the least changed, by the various degrees of heat, and of cold, to which they were exposed.

If the bottles were weighed at a time when their contents were not *precisely of the same temperature*, they would frequently appear to have gained, or to have lost, something of their weights;—but this doubtless arose from the vertical currents which they caused in the atmosphere, upon being heated or cooled in it; or to unequal quantities of moisture attached to the surfaces of the bottles;—or to both these causes operating together.

As I knew that the conducting power of mercury, with respect to heat, was considerably greater than either that of water, or that of spirit of wine, while its capacity for receiving heat is much less than that of either of them, I did not think it necessary to inclose a thermometer in the bottle C, which contained the mercury; for it was evident, that when the contents of the other two bottles should appear, by their thermometers, to have arrived at the temperature of the medium in which they were exposed, the contents of the bottle C could not fail to have acquired it also, and even to have arrived at it before them; for, the time taken up in the heating or in the cooling of any body, is, *cæteris paribus*, as the capacity of the body to receive and retain heat, *directly*, and as its conducting power, *inversely*.

The bottles were suspended to the balance by silver wires, about two inches long, with hooks at the ends of them; and, in removing and changing the bottles, I took care not to touch the glass. I likewise avoided, upon all occasions, and particularly in the cold room, coming near the balance with my breath, or touching it, or any part of the apparatus, with my naked hands.

Having determined that water does not acquire or lose any weight, upon being changed from a state of *fluidity* to that of *ice*, and *vice versâ*, I shall now take my final leave of a subject which has long occupied me, and which has cost me much pains and trouble; being fully convinced, (from the results of the above mentioned experiments,) that if heat be in fact a *substance*, or matter,—a fluid *sui generis*, as has been supposed,—which, passing from one body to another, and being accumulated, is the immediate cause of the phænomena we observe in heated bodies, (of which, however, I cannot help entertaining doubts,) it must be something so infinitely rare, even in its most condensed state, as to baffle all our attempts to discover its gravity. And, if the opinion which has been adopted by many of our ablest philosophers, that heat is nothing more than an intestine vibratory motion of the constituent parts of heated bodies, should be well founded, it is clear that the weights of bodies can in no wise be affected by such motion.

It is, no doubt, upon the supposition that heat is a substance distinct from the heated body, and which is accumulated in it, that all the experiments which have been undertaken, with a view to determine the weight which bodies have been supposed to gain, or to lose, upon being heated or cooled, have been made; and, upon this supposition (but without, however, adopting it entirely, as I do not conceive it to be sufficiently proved,) all my researches have been directed.

The experiments with *water*, and with *ice*, were made in a manner which I take to be perfectly unexceptionable;—in which no foreign cause whatever could affect the results of them;—and the quantity of heat which water is known to part with, upon being frozen, is so considerable, that if this loss has no effect upon its apparent weight, it may be presumed that we shall never be able to contrive an experiment by which we can render the weight of heat sensible.

Water, upon being frozen, has been found to lose a quantity of heat amounting to 140 degrees of FAHRENHEIT'S thermometer; or,—which is the same thing,—the heat which a given quantity of water, previously cooled to the temperature of freezing, actually loses, upon being changed to ice, if it were to be imbibed and retained by an equal quantity of water, at the given temperature, (that of freezing,) would heat it 140 degrees, or would raise it to the temperature of (32° + 140) 162° of FAHRENHEIT's thermometer, which is only 60° short of that of boiling water; consequently, any given quantity of water, at the temperature of freezing, upon being actually frozen, loses almost as much heat as, added to it, would be sufficient to make it boil.

It is clear, therefore, that the difference in the quantities of heat contained by the water in its fluid state, and heated to the temperature of 61° F, and by the ice, in the experiments before mentioned, was *at least* nearly equal to that between water in a state of boiling, and the same at the temperature of freezing.

But this quantity of heat will appear much more considerable, when we consider the great capacity of water to contain heat, and the great apparent effect which the heat that water loses upon being frozen would produce, were it to be imbibed by, or communicated to, any body whose power of receiving and retaining heat is much less.

The capacity of water to receive and retain heat,—or what has been called its specific quantity of latent heat,—has been found to be to that of gold as 1000 to 50,—or as 20 to 1; consequently, the heat which any given quantity of water loses upon being frozen,—were it to be communicated to an equal weight of gold, at the temperature of freezing, the gold, instead of being heated 162 degrees, would be heated 140 × 20 = 2800 degrees, or, would be raised to a *bright red heat.*

It appears therefore to be clearly proved, by my experiments, that a quantity of heat equal to that which 4214 grains (or about 9¾ oz.) of gold would require to heat it from the temperature of freezing water to be *red hot*, has no sensible effect upon a balance capable of indicating so small a variation of weight as that of $\frac{1}{1000000}$ part of the body in question; and, if the weight of gold is neither augmented nor lessened by *one millionth part*, upon being heated from the point of *freezing water* to that of a *bright red heat*, I think we may very safely conclude, that ALL ATTEMPTS TO DISCOVER ANY EFFECT OF HEAT UPON THE APPARENT WEIGHTS OF BODIES, WILL BE FRUITLESS.

# XII. *An Account of some Experiments on the Fecundation of Vegetables. In a Letter from* Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read May 9, 1799.

Elton, April 25, 1799.

THE result of some experiments which I have amused myself in making on plants, appearing to me to be interesting to the naturalist, by proving the existence of superfœtation in the vegetable world, and being likely to conduce to some improvements in agriculture, I have taken the liberty to communicate them to you.

The breeders of animals have very long entertained an opinion, that considerable advantages are obtained by breeding from males and females not related to each other. Though this opinion has lately been controverted, the number of its opposers has gradually diminished; and I can speak from my own observation and experience, that animals degenerate, in size at least, on the same pasture, and in other respects under the same management, when this process of crossing the breed is neglected.

The close analogy between the animal and vegetable world, and the sexual system equally pervading both, induced me to suppose, that similar means might be productive of similar effects in each; and the event has, I think, fully justified this opinion. The principal object I had in view, was to obtain new and improved varieties of the apple, to supply the place of those which have become diseased and unproductive, by having been cultivated beyond the period which nature appears to have assigned to their existence. But, as I foresaw that several years must elapse, before the success or failure of this process could possibly be ascertained, I wished, in the interval, to see what would be its effects on annual plants. Amongst these, none appeared so well calculated to answer my purpose as the common pea; not only because I could obtain many varieties of this plant, of different forms, sizes, and colours; but also, because the structure of its blossom, by preventing the ingress of insects and adventitious farina, has rendered its varieties remarkably permanent. I had a kind growing in my garden, which, having been long cultivated in the same soil, had ceased to be productive, and did not appear to recover the whole of its former vigour, when removed to a soil of a somewhat different quality; on this, my first experiment, in 1787, was made. Having opened a dozen of its immature blossoms, I destroyed the male parts, taking great care not to injure the female ones; and, a few days afterwards, when the blossoms appeared mature, I introduced the farina of a very large and luxuriant gray pea into one half of the blossoms, leaving the other half as they were. The pods of each grew equally well; but I soon perceived, that in those into whose blossoms the farina had not been introduced, the seeds remained nearly as they were before the blossoms expanded, and in that state they withered. Those in the other pods attained maturity, but were not in any sensible degree different from those afforded by other plants of the same variety; owing, I imagine, to the external covering of the seed (as I have found in other plants) being furnished entirely by the female. In the succeeding spring, the difference, however, became extremely obvious; for the plants from them rose with excessive luxuriance, and the colour of their leaves and stems clearly indicated, that they had all exchanged their whiteness for the colour of the male parent: the seeds produced in autumn were dark gray. By introducing the farina of another white variety, (or, in some instances, by simple culture,) I found this colour was easily discharged, and a numerous variety of new kinds produced, many of which were, in size, and in every other respect, much superior to the original white kind, and grew with excessive luxuriance, some of them attaining the height of more than twelve feet. I had frequent occasion to observe, in this plant, a stronger tendency to produce purple blossoms; and coloured seeds, than white ones; for, when I introduced the farina of a purple blossom into a white one, the whole of the seeds in the succeeding year became coloured; but, when I endeavoured to discharge this colour, by reversing the process, a part only of them afforded plants with white blossoms; this part sometimes occupying one end of the pod, and being at other times irregularly intermixed with those which, when sown, retained their colour. It may perhaps be supposed, that something might depend on the quantity of farina employed; but I never could discover, in this, or in any other experiment, in which superfœtation did not take place, that the largest or smallest quantity of farina afforded any difference in the effect produced.

The dissimilarity I observed in the offspring afforded by different kinds of farina, in these experiments, pointed out to me an easy method of ascertaining whether superfœtation (the existence of which has been admitted amongst animals) could also take place in the vegetable world. For, as the offspring of a white pea is always white, unless the farina of a coloured kind be introduced into the blossom, and, as the colour of the gray one is always transferred to its offspring, though the female be white, it readily occurred to me, that if the farina of both were mingled, or applied at the same moment, the offspring of each could be easily distinguished.

My first experiment was not altogether successful; for the offspring of five pods (the whole which escaped the birds) received their colour from the coloured male. There was, however, a strong resemblance to the other male, in the growth and character of more than one of the plants; and the seeds of several, in the autumn, very closely resembled it in every thing but colour. In this experiment, I used the farina of a white pea, which possessed the remarkable property of shrivelling excessively when ripe; and, in the second year, I obtained white seeds, from the gray ones above mentioned, perfectly similar to it. I am strongly disposed to believe, that the seeds were here of common parentage; but I do not conceive myself to be in possession of facts sufficient to enable me to speak with decision on this question.

If, however, the female afford the first organised atom, and the farina act only as a stimulus, it appears to me by no means impossible, that the explosion of two vesicles of farina, at the same moment, (taken from different plants,) may afford seeds (as I have supposed) of common parentage; and, as I am unable to discover any source of inaccuracy in this experiment, I must believe this to have happened.

Another species of superfœtation (if I have justly applied that term to a process in which one seed appears to have been the offspring of two males) has occurred to me so often, as to remove all possibility of doubt as to its existence. In 1797, the year after I had seen the result of the last mentioned experiment, having prepared a great many white blossoms, I introduced the farina of a white and that of a gray pea, nearly at the same moment, into each; and as, in the last year, the character of the coloured male had prevailed, I used its farina more sparingly than that of the white one; and now almost every pod afforded plants of different colours. The majority, however, were white; but the characters of the two kinds were not sufficiently distinct to allow me to judge with precision, whether any of the seeds produced were of common parentage or not. In the last year, I was more fortunate: having prepared blossoms of the little early frame pea, I introduced its own farina, and immediately afterwards that of a very large and late gray kind, and I sowed the seeds thus obtained in the end of the last summer. Many of them retained the colour and character of the small early pea, not in the slightest degree altered, and blossomed before they were eighteen inches high; whilst others, (taken from the same pods,) whose colour was changed, grew to the height of more than four feet, and were killed by the frost, before any blossoms appeared.

It is evident, that in these instances superfœtation took place; and it is equally evident, that the seeds were not all of common parentage. Should subsequent experience evince, that a single plant may be the offspring of two males, the analogy between animal and vegetable nature may induce some curious conjecture, relative to the process of generation in the animal world.

In the course of the preceding experiments, I could never observe that the character, either of the male or female, in this plant, at all preponderated in the offspring; but, as this point appeared interesting, I made a few trials to ascertain it. And, as the foregoing observations had occurred in experiments made principally to obtain new and improved varieties of the pea, for garden culture, I chose, for a similar purpose, the more hardy varieties usually sown in the fields. By introducing the farina of the largest and most luxuriant kinds into the blossoms of the most diminutive, and by reversing this process, I found that the powers of the male and female, in their effects on the offspring, are exactly equal. The vigour of the growth, the size of the seeds produced, and the season of maturity, were the same, though the one was a very early, and the other a late variety. I had, in this experiment, a striking instance of the stimulative effects of crossing the breeds; for the smallest variety, whose height rarely exceeded two feet, was increased to six feet; whilst the height of the large and luxuriant kind was very little diminished. By this process, it is evident, that any number of new varieties may be obtained; and it is highly probable, that many of these will be found better calculated to correct the defects of different soils and situations, than any we have at present; for, I imagine that all we now possess, have in a great measure been the produce of accident; and it will rarely happen, in this or any other case, that accident has done all that art will be found able to accomplish.

The success of my endeavours to produce improved varieties of the pea, induced me to try some experiments on wheat; but these did not succeed to my expectations. I readily obtained as many varieties as I wished, by merely sowing the different kinds together; for the structure of the blossom of this plant (unlike that of the pea) freely admits the ingress of adventitious farina, and is thence very liable to sport in varieties. Some of those I obtained were excellent; others very bad; and none of them permanent. By separating the best varieties, a most abundant crop was produced; but its quality was not quite equal to the quantity, and all the discarded varieties again made their appearance. It appeared to me an extraordinary circumstance, that, in the years 1795 and 1796, when almost the whole crop of corn in the island was blighted, the varieties thus obtained, and these only, escaped, in this neighbourhood, though sown in several different soils and situations.

My success on the apple (as far as long experience and attention have enabled me to judge from the cultivated appearance of trees which have not yet borne fruit) has been fully equal to my hopes. But, as the improvement of this fruit was the first object of my attention, no probable means of improvement, either from soil or aspect, were neglected. The plants, however, which I obtained from my efforts to unite the good qualities of two kinds of apple, seem to possess the greatest health and luxuriance of growth, as well as the most promising appearance in other respects. In some of these, the character of the male appears to prevail; in others, that of the female; and in others, both appear blended, or neither is distinguishable. These variations, which were often observable in the seeds taken from a single apple, evidently arise from the want of permanence in the character of this fruit, when raised from seed.

The results of similar experiments on another fruit, the grape, were nearly the same as of those on the apple, except that, by mingling the farina of a black and a white grape, just as the blossoms of the latter were expanding, I sometimes obtained plants, from the same berry, so dissimilar, that I had good reason to believe them the produce of superfœtation. By taking off the cups, and destroying the immature male parts, (as in the pea,) I perfectly succeeded in combining the characters of different varieties of this fruit, as far as the changes of form, and autumnal tints, in the leaves of the offspring, will allow me to judge.

Many experiments, of the same kind, were tried on other plants; but it is sufficient to say, that all tended to evince, that improved varieties of every fruit and esculent plant may be obtained by this process, and that nature intended that a sexual intercourse should take place between neighbouring plants of the same species. The probability of this will, I think, be apparent, when we take a view of the variety of methods which nature has taken to disperse the farina, even of those plants in which it has placed the male and female parts within the same empalement. It is often scattered by an elastic exertion of the filaments which support it, on the first opening of the blossom; and its excessive lightness renders it capable of being carried to a great distance by the wind. Its position within the blossom, is generally well adapted to place it on the bodies of insects; and the villous coat of the numerous family of bees, is not less well calculated to carry it. I have frequently observed, with great pleasure, the dispersion of the farina of some of the grasses, when the sun had just risen in a dewy morning. It seemed to be impelled from the plant with considerable force; and, being blue, was easily visible, and very strongly resembled, in appearance, the explosion of a grain of gunpowder. An examination of the structure of the blossoms of many plants, will immediately point out, that nature has something more in view, than that its own proper males should fecundate each blossom; for the means it employs are always those best calculated to answer the intended purpose. But the farina is often so placed, that it can never reach the summit of the pointal, unless by adventitious means; and many trials have convinced me, that it has no action on any other part of it. In promoting this sexual intercourse between neighbouring plants of the same species, nature appears to me to have an important purpose in view; for, independent of its stimulative power, this intercourse certainly tends to confine within more narrow limits, those variations which accidental richness or poverty of soil usually produces. It may be objected, by those who admit the existence of vegetable mules, that, under this extensive intercourse, these must have been more numerous; but my total want of success, in many endeavours, to produce a single mule plant, makes me much disposed to believe that hybrid plants have been mistaken for mules; and to doubt (with all the deference I feel for the opinions of LINNÆUS and his illustrious followers) whether nature ever did, or ever will, permit the production of such a monster. The existence of numerous mules in the animal world, between kindred species, is allowed; but nature has here guarded against their production, by impelling every animal to seek its proper mate; and, amongst the feathered tribe, when, from perversion of appetite, sexual intercourse takes place between those of distinct genera,* it has, in some instances at least, rendered the death of the female the inevitable consequence. But, in the vegetable world, there is not any thing to direct the male to its proper female: its farina is carried, by winds and insects, to plants of every different genus and species; and it therefore appears to me, (as vegetable mules certainly are not common,) that nature has not permitted them to exist at all.

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* This is said to be the case with the drake and the hen.

I cannot dismiss this subject, without expressing my regret, that those who have made the science of botany their study, should have considered the improvement of those vegetables which, in their cultivated state, afford the largest portion of subsistence to mankind and other animals, as little connected with the object of their pursuit. Hence it has happened, that whilst much attention has been paid to the improvement of every species of useful animal, the most valuable esculent plants have been almost wholly neglected. But, when the extent of the benefit which would arise to the agriculture of the country, from the possession of varieties of plants which, with the same extent of soil and labour, would afford even a small increase of produce, is considered, this subject appears of no inconsiderable importance. The improvement of animals is attended with much expence, and the improved kinds necessarily extend themselves slowly; but a single bushel of improved wheat or peas, may in ten years be made to afford seed enough to supply the whole island; and a single apple, or other fruit-tree, may within the same time be extended to every garden in it. These considerations have been the cause of my addressing the foregoing observations to you at this time; for it was much my wish to have ascertained, before I wrote to you, whether in any instance a single plant can be the offspring of two male parents. The decision of that question must of necessity have occupied two years, and must therefore be left to the test of future experiment.

# XIII. *Observations on the different Species of Asiatic Elephants, and their Mode of Dentition. By* John Corse, *Esq. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read May 23, 1799.

Having already communicated some particulars respecting the manners and habits of the elephant, which have been favourably received by the Royal Society, I trust the following observations, chiefly on the dentition of this useful animal, will not be deemed unworthy of their attention.

Before I enter on this new and curious subject, I think it will be proper to premise a few general observations on the various casts[*] of the Asiatic elephant, and also on the tusks; as the form and size of these give a diversity of appearance, which may be considered as forming varieties of the same species of elephant.

Both males and females are divided into two casts, by the natives of Bengal, *viz.* the *koomareah*[†] and the *merghee*;[‡] and this, without any regard to the appearance, shape, or size of the tusks in the male, as these serve merely to characterize some varieties in the species. The *koomareah* is a deep-bodied, strong, compact elephant, with a large trunk, legs short, but thick, in proportion to the size of the animal. The *merghee* cast, when full grown, is generally taller than the former, but has not so compact a form, nor is he so strong, or so capable of bearing fatigue; his legs are long, he travels fast, has a lighter body, and his trunk is both short and slender, in proportion to his height.

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[*] *Zat* is the word used by the natives.

[†] *Koomareah* signifies of a princely race; being derived from *koomārah*, a prince, or king's son.

[‡] *Merghee*, properly *mrigee*, from *mrigah*, a deer, or hunting, signifies an elephant used in hunting; or it is so called from its slender make.

A large trunk is always esteemed a great beauty in an elephant; so that the *koomareah* is preferred, not only for this, but for its superior strength, by which it can undergo greater fatigue, and carry heavier loads, than the *merghee*.

As there appears, however, no predilection in any of these elephants to have connection with his own particular kind, from an indiscriminate intercourse several varieties are produced, partaking of the qualities of their respective progenitors. This mixed breed is in greater or less estimation, in proportion as it partakes of the qualities of the *koomareah*, or *merghee* cast.

A breed from a pure *koomareah* and *merghee* is termed *sunkareah*,* or *mergha-bauliah*;† but, a further mixture or crossing of the breed, renders it extremely difficult for the hunters to ascertain the variety.

Besides the *koomareah, merghee*, and *sunkareah* breeds, several varieties are generally to be found in the same herd; but, the nearer an elephant approaches to the true *koomareah* species, the more he is preferred, especially by the natives, and the higher price he will consequently bear.

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* *Sunkareah* signifies a mixed breed, from *sunkarah*, a mixture.

† *Mergha-bauliah* signifies for the most part *merghee*; that is, partaking more of this cast than of the *koomareah*.

Europeans are not so particular, and will sometimes prefer a *merghee* female for hunting and riding on, when she is known to have remarkably good paces, and to be of a mild and tractable disposition.

The elephants for the service of the Honourable East India Company, are generally taken in the provinces of Chittigong and Tiperah; but, from what I have heard, those to the southward of Chittigong, in the Burmah territories and kingdom of Pegu, are of a superior breed. In confirmation of this opinion, I may observe, that the elephants taken to the south of the Goomty river, which divides the province of Tiperah from east to west, are generally better than those taken to the north of that river; and, though elephants are taken at Pilibet, as far north as latitude 29°, in the Vizier of Oude's territories, yet the Vizier, and also the officers of his court, give those taken in Chittigong and Tiperah a decided preference, they being much larger and stronger than the Pilibet elephant.

Till the year 1790, Tiperah was a part of the Chittigong province; and, so sensible was the Bengal government of the superiority of the southern elephants, for carrying burdens, enduring fatigue, and being less liable to casualties, that, in the late contracts for supplying the army with those useful animals, the contractor was bound not to send any elephant to the military stations, taken north of the Chittigong province.

Hence we may conclude the torrid zone to be the natural clime, and the most favourable for producing the largest, the best, and the hardiest elephant; and that, when this animal migrates beyond the tropics, the species degenerates. On the coast of Malabar, elephants are taken as far north as the territories of the Coorgah Rajah; but these are much inferior to the Ceylon elephant, and, from this circumstance, the report of the superiority of the Ceylon elephant to all others has probably originated. Most of the accounts we have had respecting the Asiatic elephant, have been given by gentlemen who resided many years ago on the coast of Malabar or Coromandel; where, at that time, they had but few opportunities of seeing the Chittigong or Pegu elephant.

After premising these general observations, I may here observe, that elephants have two tusks, in the upper jaw only; but those in some of the females are so small as not to appear beyond the lip, whilst in others they are almost as large as in one variety of the male, named *mooknah.*[*]

Elephants have no incisores or cutting teeth; and the grinders are so much alike in males and females, that one description will serve for both.

The largest tusks, from which the best ivory is supplied, are taken from that species of male named *dauntelah,*[†] in consequence of his large tusks, and whose countenance, from this circumstance, is the most opposite, in appearance, to that of the *mooknah;* which, as I have just observed, is hardly to be distinguished, by his head, from a female elephant. Though there is a material difference in the appearance of a *mooknah* and a *dauntelah,* as well as in the value of the tusks, yet, if they are of the same cast, (*zat,*) size, and disposition, and perfect, that is, free from any defect or blemish, there is scarcely any difference in their price.

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[*] Probably from *mookh,* the mouth or face.

[†] *Dauntelah* signifies toothy; having large or fine teeth.

An elephant is said to be perfect, when his ears are large and rounded, not ragged or indented at the margin; his eyes of a dark hazle colour, free from specks; the roof of his mouth, and his tongue, without dark or black spots of any considerable size; his trunk large, and his tail long, with a tuft of hair reaching nearly to the ground. There must be five nails on each of his fore feet, and four on each of the hind ones, making eighteen in all; his head well set on, and carried rather high. The arch or curve of his back rising gradually from the shoulder to the middle, and thence descending to the insertion of the tail; and all his joints firm and strong. There are several other points, of less consequence, which are taken notice of by the natives as well as Europeans.

The *dauntelah* is generally more daring, and less manageable, than the *mooknah;* for this reason, until the temper and disposition of the two species are ascertained, Europeans will prefer the *mooknah;* but the natives, who are fond of show, generally take their chance, and prefer the *dauntelah;* which, when known to be of a mild and gentle disposition, will always be preferred, both by Europeans and natives.

The varieties between the *mooknah* and *dauntelah* are considerable, and for these there are appropriate names, according as the form of the tusks varies from the projecting horizontal, but rather elevated, curve of the *pullung daunt** of the perfect

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* *Pullung* signifies a bed or cot, and *daunt,* teeth; and, from the tusks projecting so regularly, and being a little curved and elevated at the extremities, the natives suppose a man might lie on them at his ease, as on a bed.

*dauntelah*, to the nearly straight tusks of the *mooknah*, which point directly downwards.

When a *dauntelah* has never had but one tusk, and this of the *pullung* sort, he is said to be a *goneish* or *ganesa*,* and will sell to the Hindoo princes for a very high price, to be kept in state, and worshipped as a divinity. I have seen elephants apparently of this kind; but, when accurately examined, the tusk wanting appeared to me to have been lost by accident, so that I cannot say I ever saw a male which had originally only one tusk.

A second variety of the *dauntelah* is, when the large tusks point downwards, projecting only a little way beyond the trunk; he is then said to have *soor* or *choor daunt*.†

A third variety is the *puttel-dauntee*, whose tusks are straight, like those of the *mooknah*, only much longer, and thicker.

A fourth variety is the *ankoos-dauntee*,‡ where one tusk grows nearly horizontal, like the *pullung-daunt*, and the other like the *puttel-daunt*.

Besides these, the elephant-keepers notice other varieties, which are less distinct.

All these tusks, in the male, are fixed very deep in the upper jaw; and the root or upper part, which is hollow and filled with a core, goes as high as the insertion of the trunk, round the margin of the nasal opening to the throat; which opening is just below the protuberance of the forehead.

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* *Ganesa* is the name of the Hindoo god of wisdom, who is represented with a head like an elephant's, with only one tooth. (See Asiatic Researches, Vol. I. Art. On the Gods of Greece, Italy, and India.)

† *Soor* or *choor-daunt* signifies hogs' teeth; from the tusks having some distant resemblance to those in the lower jaw of the hog.

‡ *Ankoos* signifies a crook, and is particularly applied to the weapon the drivers use to govern the elephant, to which these irregular tusks bear some resemblance.

Through this opening the elephant breathes, and by its means he sucks up water into his trunk; between it and the roots of the tusks there is only a thin bony plate.

The first or milk tusks of an elephant never grow to any size, (Tab. V. Fig. 1.) but are shed between the first and second year, when not two inches in length. (Fig. 2.) These, as well as the first grinders, are named by the natives *dood-kau-daunt*, which literally signifies milk teeth. The tusks which are shed have a considerable part of the root or fang absorbed before this happens; as may be seen, by comparing one that has been shed, with another lodged in the socket of a young elephant, before it had cut the gum. (Fig. 1 and 2.)

The time at which the tusks cut the gum, varies considerably. I have known a young one get his tusks when about five months old; whereas, the tusks of another did not cut the gum till he was seven months old. Those tusks which are deciduous, are perfect, and without any hollow in the root, in a fœtus which is come to its full time; at this period, the socket of the permanent tusk begins to be formed, on the inner side of the deciduous tusk. (Fig. 1.)

A young elephant shed one of his milk tusks on the 6th of November, 1790, when near thirteen months old, and the other on the 27th of December, when above fourteen months old: they were merely two black-coloured stumps, when shed; but, two months afterwards the permanent ones cut the gum, and, on the 19th of April, 1791, they were an inch long, but black and ragged at the ends. When they became longer, and projected beyond the lip, they soon were worn smooth, by the motion and friction of the trunk.

Another young elephant did not shed his milk tusks till he was sixteen months old; which proves that there is considerable variety in the time at which this happens.

The permanent tusks of the female are very small, in comparison with those of the male, and do not take their rise so deep in the jaw; but they use them as weapons of offence, in the same manner as the male named *mooknah,* that is, by putting their head above another elephant, and pressing their tusks down into the animal.

These tusks are never shed, and sometimes grow to a very large size in the male. The largest I have known in Bengal, did not exceed 72 pounds, avoirdupois: at Tiperah, they seldom exceed 50 pounds; but both these weights are very inferior to that of the tusks brought from other places to the India House, where I have seen some near 150 pounds each. From what part of Asia they came, I could not learn, but suspect they were imported from Pegu to Calcutta, and thence to London.

The African elephant is said to be smaller than the Asiatic; yet I am credibly informed, by the ivory-dealers in London, that the largest tusks generally come from Africa, and are of a better texture, and less liable to turn yellow, than the Indian ivory, after being manufactured. This probably is owing to the tusks having lain longer in Africa, before they were imported, than those brought from Asia. In the latter country, most of the tusks exported are taken from elephants immediately after their death; whereas, the Africans find many teeth in the desert places which have been frequented by this animal. The intense heat of a vertical sun will undoubtedly render the ivory firmer and harder, if the tusks happen to lie on the scorching sand, or in any other dry situation.

The increase of the tusk arises from circular layers of ivory, applied internally, from the core on which they are formed, similar to what happens in the growth of the horns of some animals. When the tusks of the living elephant are sawn through, and the remaining portion exposed some months to the air, this structure is clearly shown.

If the period in which one of these circular layers is completed could be ascertained, this might lead us to fix, with tolerable precision, the age of an elephant, by counting the circles in each tusk.

Cutting off a portion of the tusks of a living elephant, is a common practice; it is done with a view to make the tusks grow thicker, when they are too long and slender, and also sometimes for the sake of uniformity, when they grow in a wrong direction.

In describing the structure of the grinders, it must be observed, that a grinder is composed of several distinct laminæ or teeth, each covered with its proper enamel; and that these teeth are merely joined to each other by an intermediate softer substance, acting like a cement. I accordingly use the words teeth, strata, layers, and laminæ, as synonimous, when speaking of the structure of the grinders.

The structure of the grinders, even from the first glance, must appear very curious, being composed of a number of perpendicular laminæ, which may be considered as so many teeth; each covered with a strong enamel, and joined to one another by the common osseous matter. This, being much softer than the enamel, wears away faster by the mastication of the food; and, in a few months after some of these teeth cut the gum, the enamel remains considerably higher, so that the surface of each grinder soon acquires a ribbed appearance, as if originally formed with ridges: this however is not the case, as may be seen by examining a grinder just cutting the gum, (Tab. VII. X. and XII.) but both its sides have a fluted form, owing to the junction of the different strata. (Tab. VI. VIII. IX. X. and XI.)

These strata, when first formed, have no firm attachment to each other, but always appear separate and distinct, when contained in their bony sockets within the jaw, after their membranes and soft parts are destroyed. Before any part of a grinder cuts the gum, there is a bony crust formed above the enamel, which gives a smoothness to the grinding surface. (Tab. VII. and XII.)

But, after the grinders cut the gum, and the convex surface has been worn down a little by the trituration of the food, each lamina appears to have been formed on several points,* which are covered by a strong enamel. There are from four to eight of these points, joined together by the common bony matter, which fills up the space between the enamelled portions.

When the grinder, however, is farther advanced in the mouth, its foremost laminæ are gradually worn down by the mastication of the food; and these enamelled points or denticuli disappear, one after another, till the enamel, at last, runs quite across the tooth, surrounding the central part on which it was formed, and taking the irregular indented plaited shape of the lamellæ. (Tab. VI. Fig. 2.) This bony centre, on which the enamel is formed, is harder than the matter which joins the teeth together, does not wear so fast, and consequently remains higher. Tab. VII. IX. and X. show these processes very distinctly.

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* This appearance has been observed by PATRICK BLAIR, M. D. F. R. S. who, in his *Osteographia Elephantina*, published in 1713, calls it digitations. The above work, which was put into my hands by my friend Dr. ALEXANDER MONRO, junior, since this Paper was written, contains some useful information. The ingenious author had, in several particulars, a tolerable idea of the formation and structure of the grinders; yet, far from suspecting a regular succession of them, he attempts to prove such succession to be impossible. He is equally erroneous in many other respects.

The number of teeth of which a grinder is composed, varies from four to twenty-three, according as the elephant advances in years; so that a grinder or case of teeth, in full grown elephants, is more than sufficient to fill one side of the mouth; (Tab. X. and XI.) in proportion, however, as the foremost layers are worn away, the succeeding ones come forward, to supply their places.

The denticuli of which each layer or tooth is composed, are much larger, and fewer in number, in old than in young elephants; (Tab. VI. Fig. 1 and 2. also Tab. VII. VIII. IX. X. and XI.) in consequence of this, the same number of laminæ generally fills the jaw of a young or of an old elephant; and, from three till fifty years, there are from ten to twelve teeth or laminæ in use, in each side of either jaw, for the mastication of the food. (Tab. VII. X. and XI.)

When several of the anterior teeth of which a grinder is composed have been completely formed, and each tooth covered with its proper enamel, they become firmly united (beginning at the fore part) by the intervention of the common bony matter, which gradually fills up the interstices between them.

When the bodies of several of the anterior laminæ have been connected together, the inferior edge of each becomes united, in the same manner, to the one next it, till the whole are thus gradually joined, and form a grinder or case of teeth.

As soon as the anterior part of the grinder is thus firmly united, the fangs or roots are next added: these, at first, appear in the form of a thin curtain or lamella of bone, extending backwards, along some of the anterior laminæ, at their lower edges. (Tab. VI. VII. VIII. and XI.) A fang common to the three anterior teeth, first begins to be formed (Tab. VII. and XI.) by the ossification shooting across from each side, in a circular direction, at the anterior portion of the first, and the posterior part of the third lamina. These join and become longer, assuming a conical shape: the hollow is gradually filled up by successive layers of the substance of the tooth, as the fang lengthens, till at last it becomes solid. (Tab. XII.) This, however, does not happen, till the three layers to which the fang is attached are nearly worn away. When its ossification is almost completed, another process begins to take place, which is, the absorption of the fang from its external surface. (Tab. VII. and XII.)

By the time that the anterior layers of the grinders are completely worn down, both the fangs and the alveolar processes begin to be absorbed. (Tab. VII. X. and XII.) Their places are gradually supplied by the next laminæ of the grinder, and their fangs, coming forward in a constant succession. When the last tooth of a grinder has advanced sufficiently in the jaw, to supply the place of its predecessor, the anterior tooth of the next succeeding grinder comes forward, to supply its place.

From the peculiar manner in which the grinders are supplied from behind, but never from beneath, a preceding grinder, (as is the case in the human species, and in most other animals,) it must appear evident, that an elephant can never shed his teeth; but, from this regular succession, he may, at one period, have only a single grinder in each side of either jaw; (Tab. X. and XI.) at another, there may be one and part of a succeeding grinder; (Tab. VII.) even a still greater variety in the appearance of the grinders will take place, according as the anterior one is more or less worn away, and the waste supplied by its successor.

In this manner, the growth of new teeth, to compose a succeeding grinder, and the ossification and formation of the fangs, are constantly going on, in regular succession; so that, after the second year, the mouth of the elephant is constantly filled with as many laminæ of the grinders on each side as it can hold. This process is beautifully and clearly exemplified in Tab. VI. VII. X. XI. and XII.

While the grinders thus advance forward in the mouth, in regular succession, the alveolus of each advances along with them; and, as the anterior fangs are absorbed, the same process is going on in the alveoli.

This is evident from Tab. VII. where the partition between the alveolus of the anterior and succeeding grinder, appears equally distinct as the one behind the last grinder, but will afterwards disappear, as soon as the roots of its preceding grinder are absorbed. In like manner, the partition between this and the incipient alveolus of the next succeeding grinder will come forward in the jaw, in proportion as the grinder itself comes into use.

In the partition between each alveolus there is a communication, which, in young elephants, is larger than in those farther advanced in years; (Tab. VI. and VII.) and it is probable, that this canal or sinus between the different alveoli, admits the passage of an elongation of the membrane, from the anterior to the posterior grinder.

The time requisite for the complete formation of one of these cases of teeth, constituting a grinder, varies from two to six or eight years; and, when an elephant has attained its full size, a considerable number of the anterior laminæ must be worn away, and the fangs absorbed, before the posterior ones can be sufficiently advanced to cut the gum. (Tab. VII. X. and XII.)

From the curved line in which the grinders of the upper jaw advance, it must be evident, that some of the anterior laminæ must be obliterated, before the last can come into use: this may be made to appear more clearly, by drawing lines parallel to the surface of the grinder (Tab. VII.) of the upper jaw; and is plainly shewn in Tab. X. where the three anterior laminæ are worn down to the fangs; yet there are ten of the posterior ones that cannot (as is evident from their convex surface) come into action, till the same number of their predecessors are worn away in regular succession. Before this could have happened, several years must have elapsed, during which, the posterior laminæ would have been completed; for, in the present state, the three aftermost layers are not even now attached to each other, or to the rest which are anterior; the membrane between, and connecting these laminæ, not being ossified at the time of the animal's death. In this grinder, there are twenty-three laminæ, which is the greatest number I have seen.

In the lower jaw, the same circumstances take place; the teeth of the grinders rise by the addition of their fangs, force their way through the alveoli, and cut the gum, as they advance forward in the jaw. The grinding surface has rather a concave form, to adapt itself to that of the grinder in the upper jaw. The number of layers does not always correspond with those of the grinder in the upper jaw; but, like them, consists of from four to about twenty-three teeth or laminæ. (Tab. VI. and XI.)

In both jaws, the alveoli are firmly attached, anteriorly and laterally, to the bony plates of which the jaw is composed; but, at the posterior part, these alveoli are separate from the jaw, and have only a membranous attachment. The alveoli terminate in an apex or point, (Tab. VII. IX. X. and XI.) and become thicker and stronger, (Tab. VI. IX. and XI.) as the elephant advances in years.

In the lower jaw, the portion of the alveolus which is attached to the inner plate, is thick and spongy; and, through the under part of this spongy substance, there is a pretty large foramen, for transmitting the blood-vessels and nerves which supply the teeth and lower jaw. The alveolus of the grinder advances in the same manner in the lower as in the upper jaw; and, as the fangs are absorbed, it is absorbed also.

In proportion as the fangs or roots are added to the grinder, it rises through the alveolus, and cuts the gum; at the same time, the bottom of the alveolus, in which the grinder is formed, becomes more spongy, and shoots up between the fangs, firmly embracing them, and thus preventing the grinder from being shaken or disturbed by the trituration of the food. (Tab. VI. VII. and XI.)

As the grinders of the upper and under jaws wear away, the roots of them are lengthened, and become more solid, by the internal addition of new matter, till the cavity is entirely filled up. (Tab. XII.)

This lengthening of the roots is necessary, to give that portion of the grinder in use sufficient firmness in the jaw, as well as to keep the surface at a proper level above the gum. That the alveolus shoots up between the roots, instead of the roots striking deeper into the sockets, as they lengthen, is obvious from Tab. VI. VII. and XI.; where, a line drawn along the points of the roots, will follow the direction of those parts of the grinder which have not any roots.

When the anterior teeth are worn down to the roots, these, with the sockets, begin to be absorbed, to make room for their successors, which are coming forwards.

The shape of a grinder of the lower jaw is very different from that of one of the upper: in the latter, the grinder advances from behind straight forwards, and the back part has a very convex shape; whereas, the lower grinder advances rather in a bent or curved direction, adapting itself to the shape of the jaw. (Tab. XI.)

The surface of this grinder is somewhat of a concave figure, adapted to the form of the corresponding grinder in the upper jaw. The upper and lower grinders, (Tab. VIII.) and the section of a grinder, (Tab. XII.) shew, in the clearest manner, the progress of ossification in the roots, and the manner in which the different teeth are joined.

In a young elephant, soon after birth, the milk grinders, with their roots, are completely formed; and, even the succeeding or second set of grinders have the roots partly added to some of the anterior teeth, which are soon to cut the gum; but the posterior layers are then without roots. (Tab. VI. Fig. 1.)

Farther back in the jaw, the third grinder, which is composed of about thirteen teeth, has no appearance of roots; nor have the different teeth any connection with each other, except by the common membranes. When these are destroyed, the teeth or rudiments of a succeeding grinder can be easily separated from each other. At this period, the enamel of the third grinder has not been formed, but only the substance of the teeth, which it afterwards covers, adapting itself to the irregularities of the surface. (Tab. VI. Fig. 2.) When a grinder is considerably worn down, these irregularities of the central lamellæ are evident, from the enamel of each tooth being indented and puckered, as it were, all round. (Tab. VII. IX. and X.)

Having thus attempted to explain, in a clear and satisfactory manner, the progressive growth and regular succession of the grinders, I will next point out the periods in which I conceive these respective changes to take place. Here, however, I am in considerable doubt and uncertainty; but will fairly state the circumstances which first drew my attention particularly to this subject, as well as the grounds upon which my conclusions have been made.

In November, 1795, I sent a couple of elephants' heads, through my friend Mr. FAIRLIE, of Calcutta, to D. SCOTT, Esq. of Upper Harley-street, to be placed by him in some public museum.* In my letter, dated the 17th of that month, I mentioned the most remarkable peculiarities of these heads, and particularly the grinders; but at the same time made this remark, "there is only one tooth in each side of either jaw, till "an elephant attains its full growth." Upon examining afterwards the heads of some younger elephants, I perceived I had made a mistake, and that there was not always only one grinder in each side of the jaw. This want of uniformity in the appearance of the grinders of young elephants, of the same size, and nearly of the same age, showed me my mistake, and puzzled me a good deal; nor did I perceive any means whereby I could satisfactorily and rationally account for it, till I had carefully compared a number of heads, of different ages, with each other.

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* These were afterwards sent to the Right Hon. Sir JOSEPH BANKS, Bart. and by him to the British Museum, where they now are.

To effect this, I immediately began to collect the heads of such elephants as died at Tiperah, with the size and qualities of which I was perfectly acquainted: in the course of the year 1796, I procured above thirty heads, and, beginning with the youngest of these, I arranged them as nearly as possible according to their respective ages.

As it may be satisfactory to many members of the Royal Society, to learn the means by which I was enabled to collect the heads of so many elephants, whose heights and qualities I had accurately ascertained, I shall just observe, that between the beginning of November, 1795, and the 1st of April, 1796, there were four herds of elephants taken in Tiperah.

Three of these herds were taken under my immediate inspection: the fourth, consisting of about fifty elephants, was taken by the Rajah's hunters, but was afterwards so terribly neglected, and almost starved to death, that I was requested by the Rajah to take them under my management; to this I consented, and his servants were ordered to obey implicitly my directions. In consequence, however, of the former ill treatment the elephants had received, above half of them died in the course of a few months; these, with some other casualties, enabled me to form the numerous collection above mentioned.

The elephants from which the heads were taken being well known to me, I was enabled to form a tolerable estimate of the ages of several of them; those young ones whose ages are particularly specified, were brought forth after their dams were secured.

After arranging and comparing the heads with each other, I endeavoured to ascertain the different periods necessary for the formation of the grinders, in young and old elephants, and thence to draw some conclusions, respecting the progress of dentition in this useful animal.

The first set of grinders, or milk teeth, begin to cut the gum eight or ten days after birth; and the grinders of the upper jaw appear before those of the lower one. Though this happens at first, yet, in a few months, the grinders in the lower jaw come forward faster than those of the upper, as I have observed in the heads of several elephants. (Tab. VII. and IX.)

In about six weeks, the first set of grinders can be easily felt, consisting of four teeth, *viz.* one on each side of either jaw; and, as young elephants begin to eat grass, or some soft succulent food, before they are three months old, we may conclude, that the first set of grinders* have then completely cut the gum, and that dentition is not attended with any symptoms of pain, or irritation, in the system.

The milk grinders are not shed, as the tusks are, but are gradually worn away, during the time the second set are coming forward; and, as soon as the body of the grinder is nearly worn away, the fangs begin to be absorbed. (Tab. VI. Fig. 3.)

I have not been able to ascertain the exact time when the second set of grinders make their appearance, as I could never get an elephant to open his mouth in such a manner as to permit me to examine his teeth accurately; but, when the elephant is about two years old, the second set are completely in use.

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* By a set, I mean four, one grinder in each side of either jaw.

At this period, the third set begin to cut the gum. From the end of the second to the beginning of the sixth year, the third set come gradually forward, as the jaw lengthens, not only to fill up this additional space, but also to supply the place of the second set, which are, during the same period, gradually worn away, and their fangs absorbed.

From the beginning of the sixth to the end of the ninth year, the fourth set of grinders come forward, to supply the gradual waste of the third set.

After this period, several other sets are produced. In what time these succeeding grinders come forward, in proportion to their predecessors, I have not been able to ascertain; but, from the data already given, I conclude, that every succeeding grinder takes at least a year more than its predecessor to be completed; consequently, that the fifth, sixth, seventh, and eighth set of grinders (a further succession I have not been able to trace) will take from five to eight years, (and probably much longer,) each set, before the posterior lamina has cut the gum.

The milk grinders consist each of four teeth or laminæ; the second set of grinders of eight or nine laminæ; the third set of twelve or thirteen; (Tab. VI. Fig. 1.) the fourth set of fifteen; and so on, to the seventh or eighth set, when each grinder consists of twenty-two or twenty-three, (Tab. X. and XI.) which is the greatest number I have observed.

All these circumstances considered, I may venture to affirm, that the formation of the teeth and mode of dentition, in the elephant, has but little analogy with those of any other quadruped; nature having, by a peculiar and wonderful contrivance, and in the most convenient manner, supplied this animal with a regular succession of teeth, until he attains a very advanced period of life. An advantage which, as far as we know, no other quadruped possesses.

The mode in which the elephant's grinders are originally formed, my short stay at Tiperah did not allow me sufficient opportunities to investigate; but, since my return to England, I have had frequent conversations with my friend Mr. HOME upon that subject, who, from an examination of the teeth brought home by me, and some preparations in the late Mr. HUNTER's collection, has been able to prosecute the subject with considerable success. His observations will be laid before the Royal Society, immediately after the present Paper, as a continuation of the same subject.

## EXPLANATION OF THE PLATES.

### Tab. V.

Fig. 1. Is intended to represent the deciduous or milk tusks of a young elephant, and the process of absorption taking place in the fang of a tusk, before it can be shed: also the incipient alveolus of the permanent tusk on the left side. To show these distinctly, part of the anterior plate of the bone is removed. On the right side, the bones of the face and deciduous tusk remain in their natural situation.

AAAA. Represents the fore part of the skull of a young female elephant, (nearly of its natural size,) which died soon after birth.

BB. The nasal opening, round which the trunk is inserted.

C. Shows the deciduous tusk *in situ*.

D. The middle of the tusk, where the process of absorption has commenced.

E. The crooked fang, which must be absorbed, before the body of the tusk can be shed.

F. The small rounded cavity on the interior side of the deciduous tusk, which is the incipient socket for the permanent one.

GG. Are two cells, one on each side of the root of the deciduous tusk, which communicate.

H. The right deciduous tusk, in its natural state.

Fig. 2. Represents the deciduous tusk of an elephant, when it has been shed.

A. The ragged point of the tusk.

B. The body of the tusk next the fang, as seen in its natural situation at D, Fig. 1. the fang having been absorbed.

## Tab. VI.

Fig. 1. Exhibits the left side of the lower jaw, of nearly its natural size, with the plate of the interior side removed, to show the first, second, and third succeeding grinders *in situ.*

A. The condyloid process.

B. The coronoid process.

C. The symphysis of the lower jaw.

D. The anterior or milk grinder, the body of which consists of four laminæ; of these, the three foremost had cut the gum.

EE. The two fangs, nearly completed, but still hollow at the points.

F. The canal for lodging the blood-vessels.

G. The canal for the passage of the larger blood-vessels that supplied the jaw and teeth, extending from below the root of the second grinder to where the interior plate of the jaw has been removed.

HH. The second grinder, no part of which had cut the gum; it consists of nine teeth.

I. The fang common to the three anterior ones, partly formed.

K. The bony lamella or incipient roots, partly added to the next three teeth.

LLL. The three posterior teeth or laminæ, which were not united to the anterior ones when the animal died.

MM. The third succeeding grinder, consisting of thirteen laminæ or rudiments of future teeth: it remains within the bony socket, and, except the two anterior laminæ, all the rest have no adhesion.

N. The alveolar process behind the grinder MM: it does not adhere to the jaw, but appears in the form of a thin bony lamella.

O. The partition between the second and aftermost grinder.

P. The opening or communication, in that partition, between the second and third grinder.

Fig. 2. Represents a tooth of the third grinder, consisting of eight points at the top, partly covered with enamel. The lower part has a puckered or plaited form, probably intended to give firmer adhesion to the enamel which is afterwards to cover it.

Fig. 3. Shows a milk grinder worn down to the fangs; part of which, and of the body of the grinder, are absorbed. Where the absorption has taken place, the grinders and roots have a rough ulcerated appearance.

### Tab. VII.

Represents the third grinder of the left side of the upper jaw, worn away almost to the fangs, even at the posterior part: the anterior part completely worn away, and great part of the roots and their alveolar processes absorbed. In this grinder, the indented puckered form of the enamel is shown, and also its ribbed surface.

Of the three teeth of the succeeding grinder, which had cut the gum, the aftermost tooth shows eight denticuli or points, each surrounded with its proper enamel: the next shows five denticuli; but the anterior tooth of this grinder has the centre completely surrounded with enamel. The appearance of this grinder is very happily represented; the posterior teeth nearest the gum being covered with a whitish bony crust above the enamel; but, nearer the roots, they are not yet joined together.

The six anterior laminæ have the roots partly added; but the eight posterior ones are entirely without any appearance of roots.

Immediately above this grinder, the incipient alveolus of a fifth succeeding grinder begins to be formed. The canal between this and the anterior alveolus, appears at the upper part of the bony partition which divides the alveoli.

The external plate of the upper jaw bone and part of the alveolus are removed, to show these grinders *in situ.*

The apex or upper part of the bony case, in which the grinders are lodged, appears distinct, and unconnected with the bones of the jaw.

AAAA. The division of the bones of the upper jaw and the alveoli, to show the grinders *in situ.*

BBB. The alveoli or sockets of the two grinders; *b*, the incipient alveolus of a succeeding grinder.

C. The communication between this and its anterior alveolus.

DD. The partitions between the alveoli.

EEEEE. The alveolar processes, which proceed from the alveolus, between the roots of the grinders.

F. The smooth bony surface of the three anterior layers of the grinder, without enamel, which has been completely worn away.

GGG. The enamel of the different teeth, surrounding their bony centres.

HHH. The centres of the teeth of which a grinder is composed.

III. The bony matter which joins the teeth to each other.

KK. The denticuli or points of the tooth, before they are much worn down; each point surrounded with its proper enamel.

A line drawn between L and L, will show that some of the anterior teeth must be worn down to the fangs, before several of the posterior ones can cut the gum.

## Tab. VIII.

Fig. 1. Shows the inferior parts of the teeth of the second grinder in the lower jaw, represented in Tab. VI. and the manner in which each tooth or lamina is joined to the one next it. The three anterior teeth are completely joined to each other, and are partly seen within their common fang; but the junction of the three posterior ones is not complete, except at the centre. The roots of these, at the lower part, are beginning to be formed; but, anteriorly, are farther advanced, and join the fang common to the three foremost teeth.

AA. The nine teeth of which the grinder is composed.

B. The three anterior teeth, completely joined to each other.

C. Three of the posterior teeth, where the union is completed only in the middle.

DDD. The space between the lamellæ of the separate teeth, where they are approaching to join each other, and cover the enamel, as shewn in Tab. XII.

EEEEE. The centres of the different teeth, which will afterwards be filled up, as the ossification of the fangs advances.

Fig. 2. Is intended to show the same process in the aftermost grinder of the upper jaw, represented in Tab. VII. The six anterior teeth are completely joined, and the roots partly added. The four next are connected at the external and internal sides only. The four aftermost layers remain quite separate and distinct from each other, where the roots are to be added. The incipient roots appear in the shape of a thin bony lamella.

A. The incipient fang of three anterior teeth.

BBB. The incipient roots of the posterior laminæ, extending along the grinder.

CC. The opposite lamellæ of two contiguous teeth, beginning to join.

DDDDD. The centres of the posterior teeth, where the lamellæ have not yet begun to join with each other.

EEEE. The divisions between the teeth, which have at first a membrane interposed, but are afterwards filled up by the common bony matter, when the enamel is completed.

### Tab. IX.

Shows part of the left side of the lower jaw of the elephant whose grinders are represented in Tab. VII. The grinders are represented nearly of their natural size. Here the fourth succeeding grinder is farther advanced than its corresponding one in the upper jaw. Six layers of this had cut the gum; whereas, only three in the former appeared through the gum. Part of the external plate of the interior side of the jaw, to which the alveolus or bony case does not adhere, is removed, to show the incipient alveolus of the fifth grinder, which, when opened, contained one denticulus.

Between this and the anterior alveolus, is distinctly seen the canal of communication, which serves to connect the membranes of each grinder. Beneath the alveolus there is a very large canal, for the transmission of the blood-vessels and nerves; which, as it advances forwards, passes through the alveolus, where this is attached to the jaw bone.

The termination of the alveolus in an apex, is very distinctly marked in this, as well as in Tab. VII.

The fluted appearance of the grinder, and also the plaited form of the enamel of each tooth, are distinctly seen.

AAAA. The divided surface of the portion of bone that was removed, to show the apex of the bony case of the grinders, and incipient alveolus of the fifth grinder.

BB. The alveolus, as far as it is unconnected with the jaw.

C. A small portion of the jaw bone, adhering inseparably to the alveolus.

D. The incipient alveolus.

E. The canal of communication between the alveoli.

FF. The uneven surface of the grinder.

GG. The fluted appearance of the grinder, by the junction of the different teeth of which it is composed.

HH. The canal for the transmission of the blood-vessels and nerves which supply the teeth and lower jaw.

## Tab. X.

Represents the seventh or eighth succeeding grinder, in the left side of the upper jaw of an elephant, in the completest state possible, though on a small scale, about half the natural size. The two anterior teeth are quite worn down to the fangs; and the three farthest back are not so far advanced as to adhere to the teeth immediately before them. The denticuli of which the teeth appear to be formed, are larger than in young elephants.

This grinder consists of twenty-three teeth. The anterior one is worn away; twelve are in use; and ten remain to come through the gum. The dark margin, round the grinding surface, shows where the gum was attached: it is also seen in Tab. VII.

By drawing lines parallel to the grinding surface of the teeth in the upper jaw, it will appear, that several of the anterior layers must be completely worn away, before the posterior teeth can cut the gum.

Above the last layer of this grinder, there appears an incipient alveolus for another succeeding grinder; though the elephant to which this head belonged appeared aged.

AAA. The line of separation between the alveolus and bones of the upper jaw.

BBB. The surface of the tooth to which the gum is attached.

C. An incipient alveolus for a succeeding grinder.

## Tab. XI.

Represents, *in situ*, a very complete grinder, about half the natural size, in the right side of the lower jaw of a full grown elephant. It consists of twenty-two layers or teeth, and the three farthest back are not yet attached to their anterior laminæ. Here the gradual progress of ossification in the fangs is distinctly seen; and, from the apex of the alveolus, to near the centre of the grinder, it is not attached to the jaw. A small portion of the jaw bone, covering the large foramen through which the apex ascends, has not been removed. In this, as well as in the last plate, the grinder completely fills the jaw, and only half of the grinder is employed for masticating the food. The anterior teeth appear evidently to rise by the addition of the fangs; which here, as well as in the grinders of the upper jaw, lengthen as the teeth wear away, till, the roots being completed, they begin to be absorbed. The anterior fangs are very distinct, but still hollow at the points. The bony lamella appears extending backwards, as the foundation of the succeeding roots.

In this and the preceding plate, the enamel and centre of each tooth appear higher than the divisions between the teeth.

A. The condyle.

B. The coronoid process.

C. The apex of the alveolus.

DDD. The section of the portion of the jaw bone and alveolus, which were removed to show the grinder *in situ.*

EE. The division between the alveolus and jaw bone, where there is only a membranous attachment.

FF. The spongy substance of the alveolus, which becomes thicker as it advances forward, and from which the alveolar processes shoot up between the roots of the teeth, in proportion as they are lengthened.

GG. The grinding surface employed in masticating the food.

HHH. Three anterior fangs, considerably lengthened, but still hollow.

II. The bony lamella, or incipient roots of the posterior teeth.

## Tab. XII.

Represents a grinder of the upper jaw, divided longitudinally, and its surface polished, to show the structure. The anterior fang is solid, almost to the extremity, and part is already absorbed, where it appears rough and ulcerated. The hollow that formerly existed between the three next teeth and their roots is filled up; and the ossification of the fangs to the bodies of the teeth, is seen proceeding regularly backwards.

The roots of the four teeth farthest back are still very hollow. The centres of the four anterior ones are completely filled up; and the progress of ossification is less complete as you go back.

The dark parts represent the divisions between each tooth: the white is the enamel; and the penniform or striated parts represent the centre of each tooth. The grinding surface appears serrated, the enamel being highest: the centre or body of the teeth next; and the bony divisions between each appear rather the lowest. The seven teeth farthest back have their enamel complete, and a bony crust laid over them; but, as they cut the gum, this is soon worn away, and the teeth themselves become gradually worn down by the mastication of the food.

By tracing the centre of a tooth from its apex to the roots, the mode in which the teeth are joined to each other will appear evident. The centres of the teeth are not entirely filled up, till the fangs become nearly solid.

From A to A are fifteen teeth, of which this grinder is made up.

B. The anterior fang, become quite solid, except at the extremity.

C. The hollow that formerly existed between the three anterior teeth and their fangs, completely ossified.

DD. The ossification extending from the fangs to the bodies of the teeth, which, in time, would have filled up the intermediate space between them.

EE. The fangs of the grinder, considerably lengthened, and nearly solid, except at the posterior part.

FFF. The bony crust covering the teeth, before they cut the gum.

GG. The serrated surface of the grinder, produced by the mastication of the food; the enamel and centre of the teeth being higher than the intermediate bony substance.

HHH. The bony matter, filling up the interstices between, and uniting the teeth. From these three, which are marked, the rest can be readily known.

III. The respective teeth or laminæ, each covered with its proper enamel. Only three of them are marked, but, from these, the rest must appear obvious.

KKK. The enamel, extending from the apex and grinding surface to the roots of the teeth, where it joins the enamel of the next tooth.

LL. Points out the manner in which the bony centres of contiguous teeth join, and cover the enamel; this junction takes place before the enamel is completed. Vide Tab. VIII. Fig. 1 and 2.

*Philos. Trans.* MDCCXCIX. *Tab.* V. *p.* 236.

*Fig. 1.*

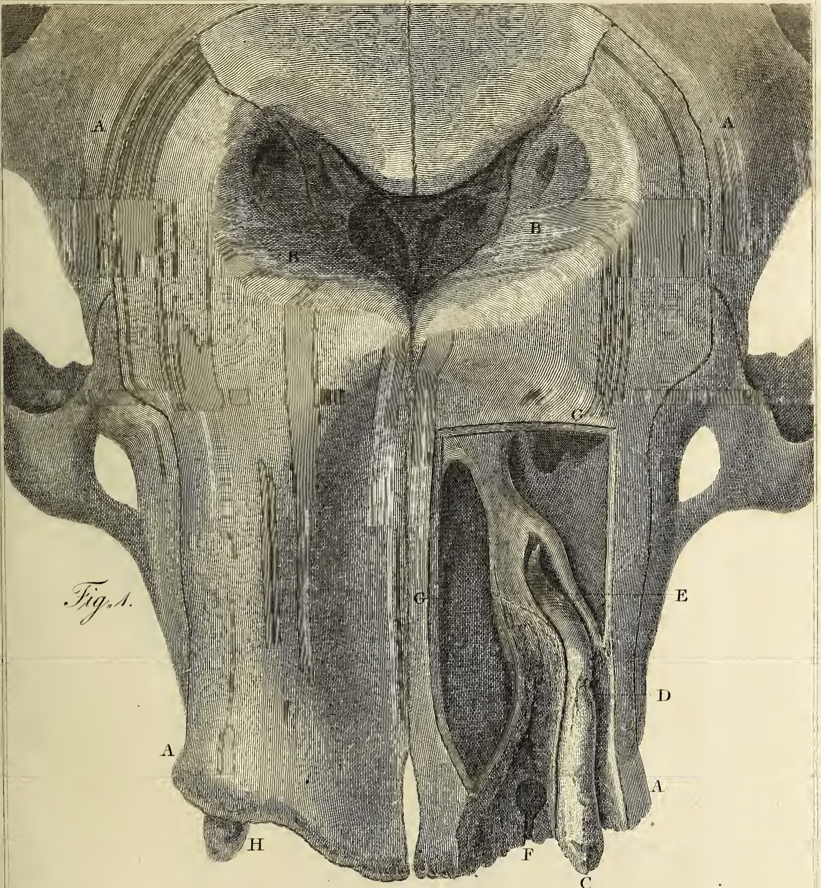

*Fig. 2.*

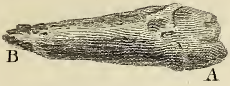

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*Fig. 3.*

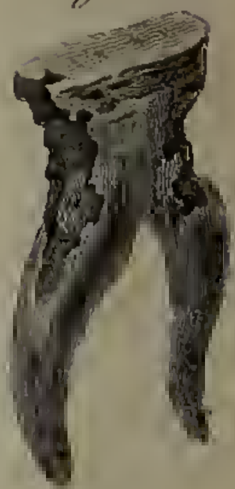


*Fig. 2.*

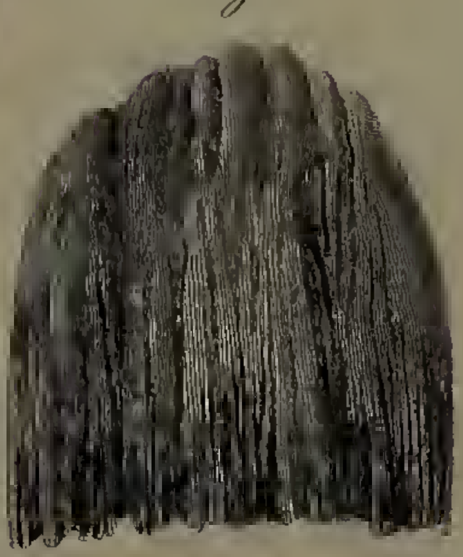
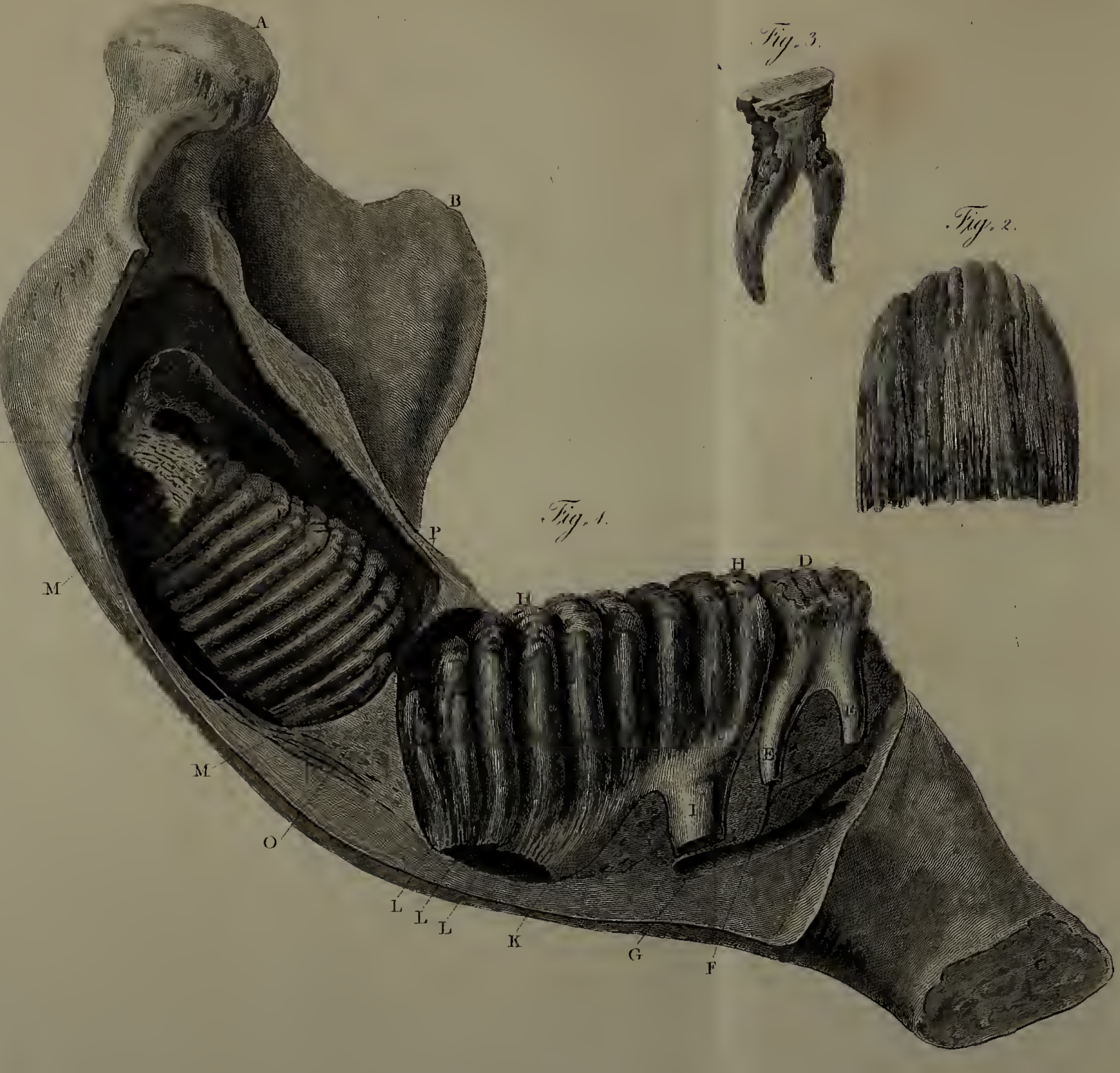


*Fig. 1.*



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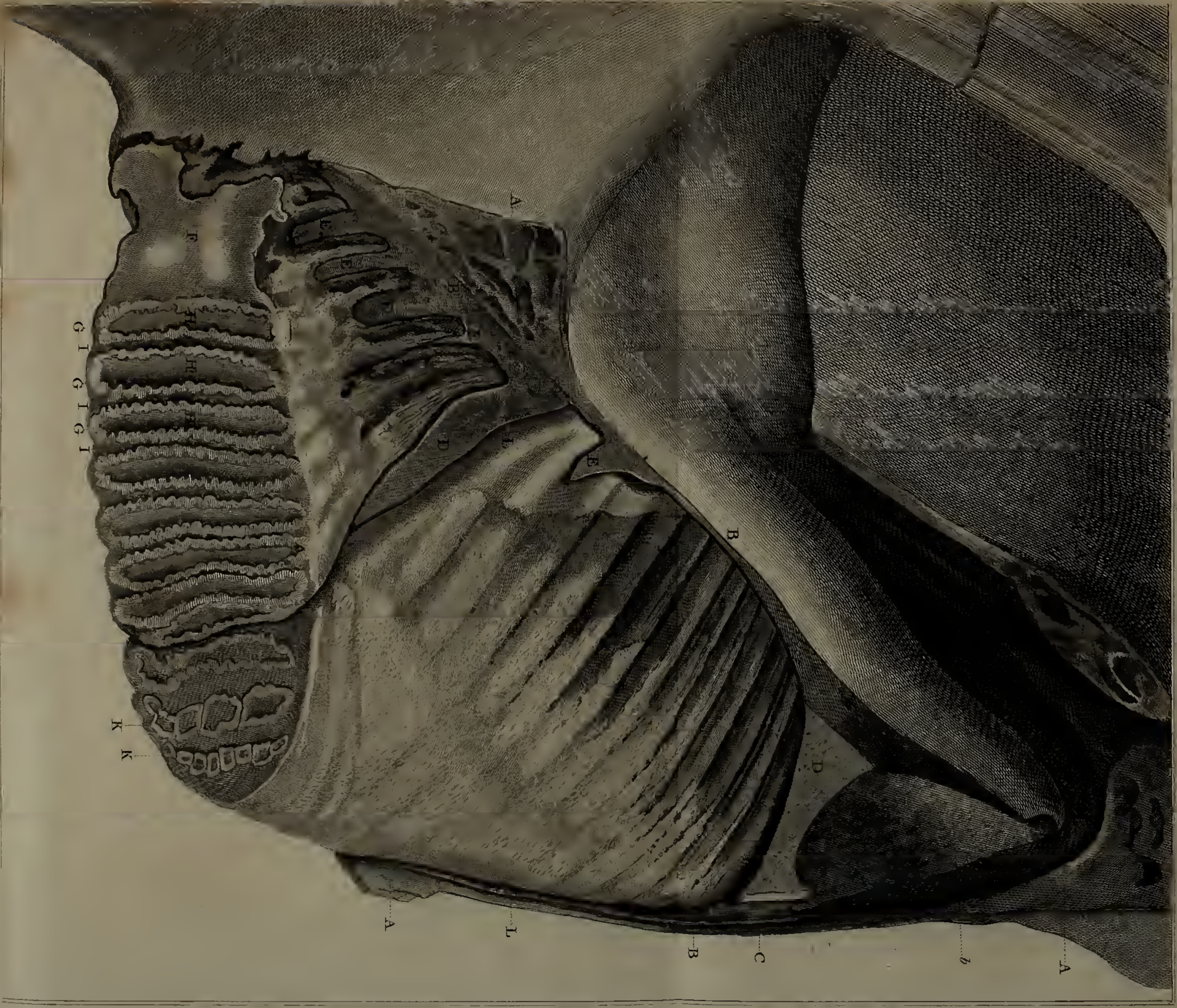

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*Fig. 1.*

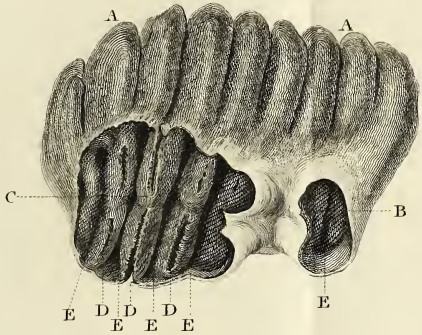
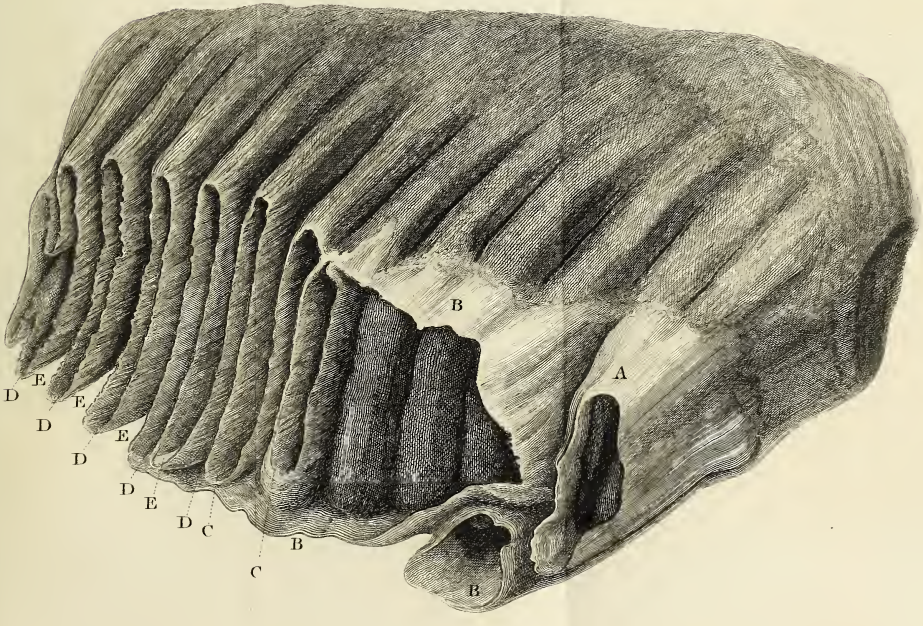


*Fig. 2.*



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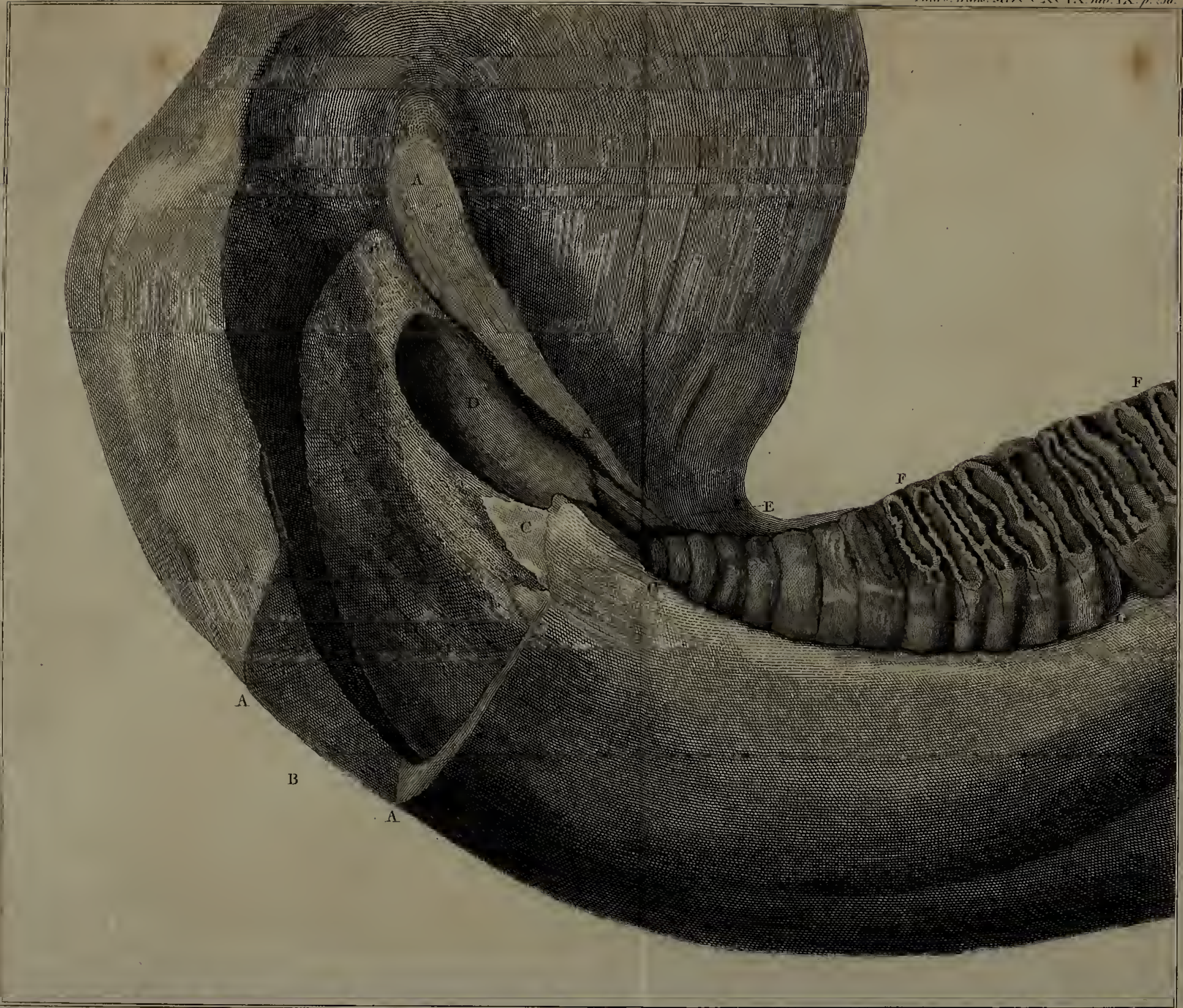

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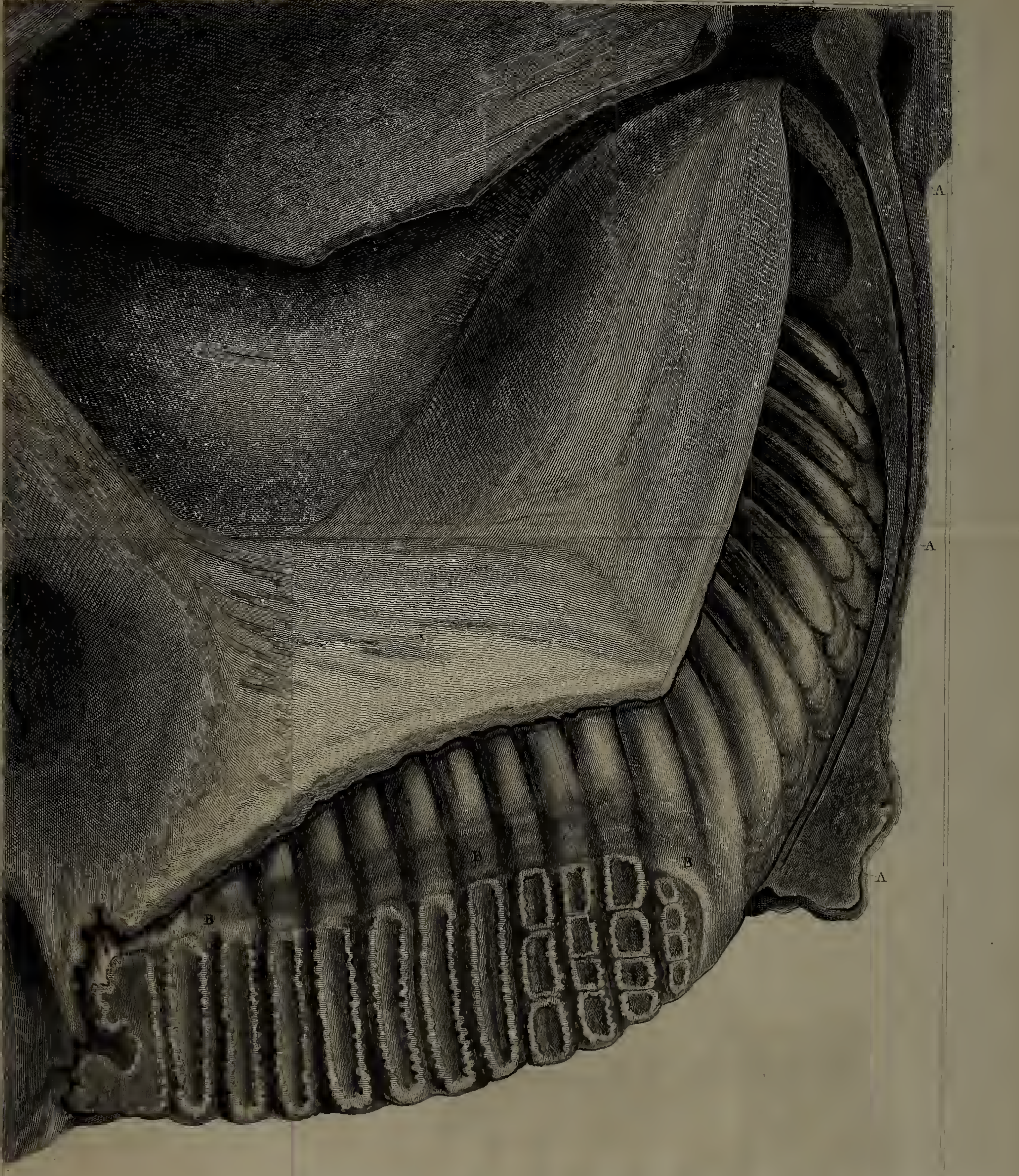

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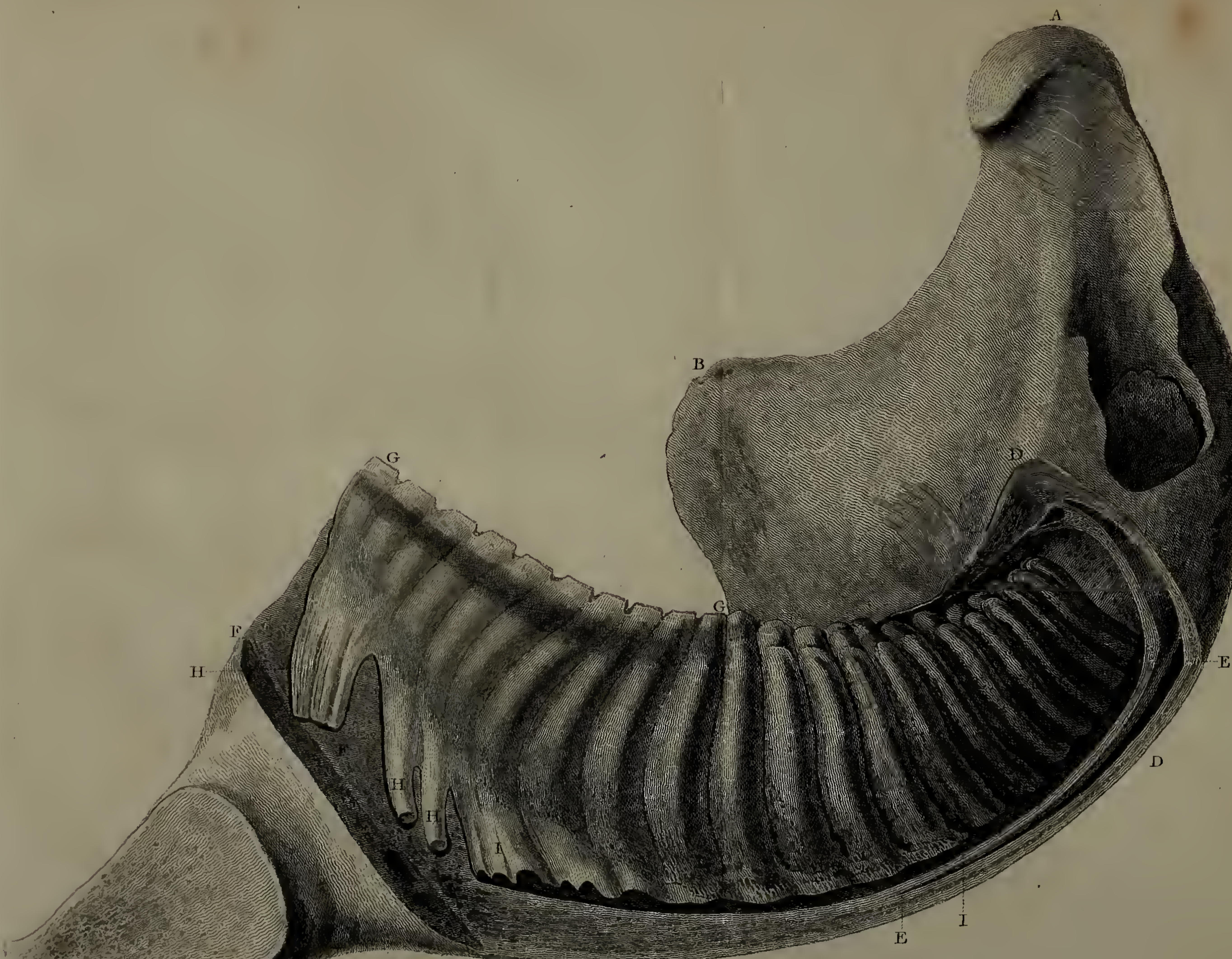

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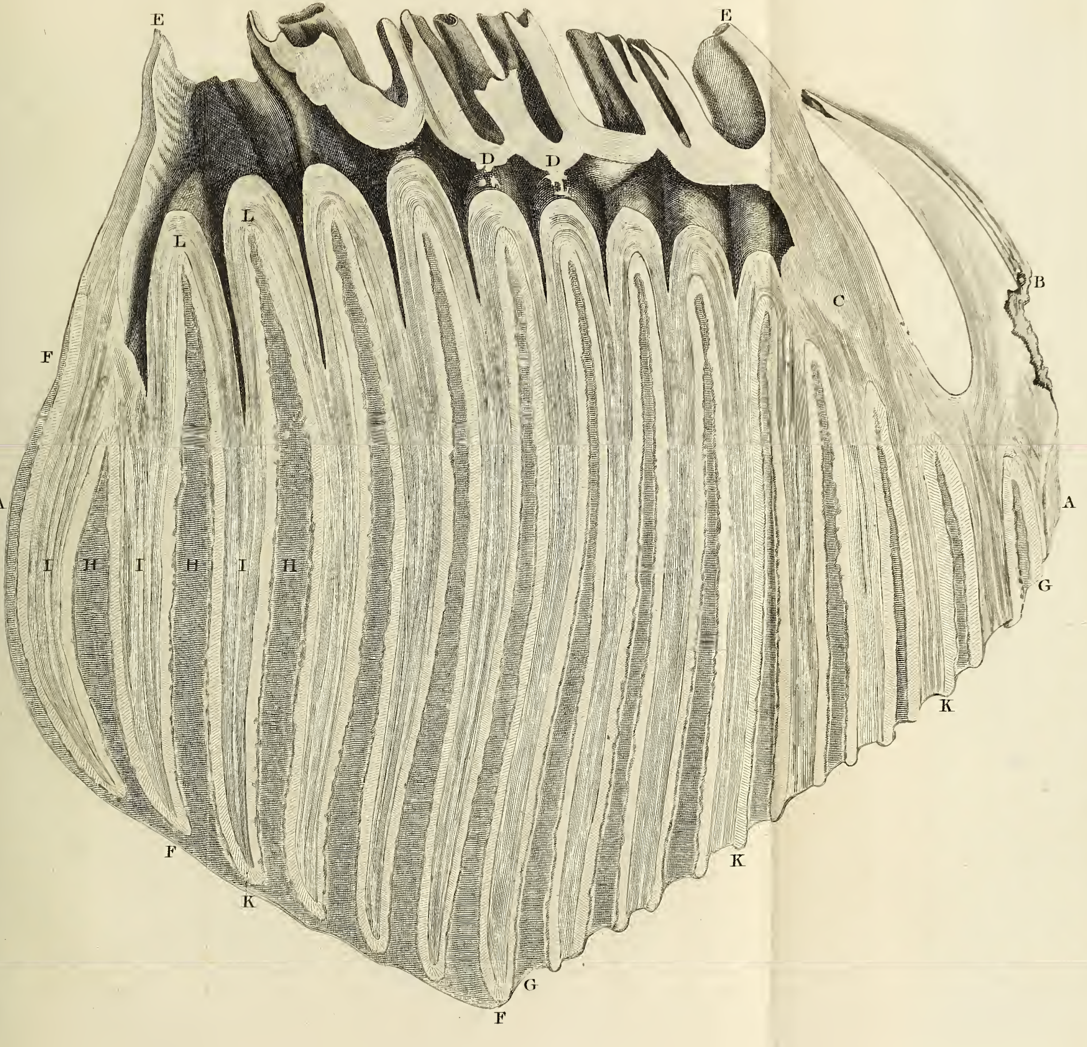

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BRITISH MUSEUM NATURAL HISTORY

# XIV. *Some Observations on the Structure of the Teeth of graminivorous Quadrupeds; particularly those of the Elephant and* Sus Æthiopicus. *By* Everard Home, *Esq. F. R. S.*

Read May 30, 1799.

When Mr. Corse did me the honour of putting into my hands his Observations on the Elephant's Teeth, and shewed me the teeth themselves in their different stages of growth, in illustration of what he had advanced upon the subject, I very readily engaged in the prosecution of so curious an investigation. I examined several specimens of elephants' teeth, preserved in spirit, while in a growing state, which are deposited in Mr. Hunter's collection of comparative anatomy, and compared them with the teeth in Mr. Corse's possession. From these two sources, I was enabled to procure every information that was required, to explain the structure of the elephant's teeth, and to point out the general principle upon which all teeth are formed, that have the enamel intermixed with the substance of the teeth; a subject, as far as I am acquainted, not hitherto investigated.

The success that has attended this inquiry may be ascribed to my opportunities of consulting Mr. Hunter's collection:—this tribute I pay with much satisfaction, and mention it here, in proof of the value of that collection, which is not confined to the discoveries and investigations of Mr. Hunter, but extends much further; since the collection contains materials relating to many subjects at present but little understood, and which, if they are hereafter rendered accessible to those engaged in such researches, will lead to the advancement of knowledge, and to many useful discoveries.

As Mr. CORSE's labours in India have enabled him to give so correct and satisfactory an account of the mode of growth of the elephant's teeth, I was desirous that the facts which I had discovered respecting their structure, should be introduced into his Paper, to render the account more complete:—this offer, Mr. CORSE however declined, not choosing to bring forward any thing that was not wholly his own; I have therefore added them in this place, as a proper supplement to Mr. CORSE's Paper.

To make my observations on the structure of the complex tooth of the elephant intelligible to this learned Society, it appears necessary to mention, generally, the mode in which the more simple teeth of the human species, and of carnivorous animals, are formed: this knowledge will render the account of such additional parts as are met with in those of the elephant, more easily understood.

The teeth of carnivorous animals are formed from a vascular pulp, of the same shape with the future tooth, upon the external surface of which the substance of the tooth begins to grow, and increases till it is completely formed. This pulp is inclosed by a capsule, the cavity of which, while the tooth is growing, is filled with a viscid fluid, similar to the synovia of joints; and this fluid, by the absorption of the thinner parts, becomes inspissated to a proper state for crystallization, so as to form the enamel, which adheres to the surface of the tooth.

Teeth formed in this way, are composed of two parts, of dissimilar texture: one, the enamel, which is striated; the other, the substance of the tooth, which is laminated, like ivory, being more compact than common bone, and less so than the enamel; but differing from both in the mode of its formation.

Bones are formed in two different ways: those that are cylindrical, have cartilage for their basis; those that are flat, either cartilage or membrane; but, in no instance in the body are they formed upon a pulp. The substance of the tooth must therefore be considered as distinct from bone, and may be ranked, both from its structure and mode of formation, as a species of ivory.*

The teeth of the elephant differ from those just described, in being composed of a great many flattened oval processes; these, while growing, are detached; but, when completely formed, their bases unite together, and make the body of the tooth, to which the fangs are afterwards added; and, as the fangs are lengthened, the tooth rises in the jaw. This is what may be considered as the tooth itself, being composed of the same materials as the teeth of carnivorous animals; but, in addition, there is another substance, which unites all the processes together, laterally, into one mass; this is softer than the substance of the tooth, and, upon examination, proves to be similar, in its texture and formation, to common bone.

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* The tusks of the elephant are formed upon a pulp, similar to teeth.

Tumors are sometimes met with in the frontal sinuses of the human body, having a perfect resemblance to ivory; they have their origin in the bony cavity of the sinus, and extend themselves into the orbit of the eye. Of these, I have seen two instances, and was unable, at the time, to account for them; but am now induced to believe they were formed upon vascular excrescences, growing from the lining of the sinuses, similar in their organization to the pulps above mentioned.

As teeth have been hitherto considered of the same texture with common bone; it is probable that nothing but the two substances being united in the same mass, could have led me to the discovery of their differing materially from each other. It will therefore be proper to explain, in this place, the circumstances which first gave me the present view of the subject.

To obtain an accurate knowledge of the different parts of the elephant's tooth, a longitudinal section was made, of one that was full grown. This section exposed the lateral connection between the different processes, and the intermediate substance which unites them into one mass; it also showed the mode in which the processes are continued into the body of the tooth and fangs.

That the internal structure might be made more distinct, the surface of this section was polished very highly, which led to the discovery of the processes of the tooth having a more compact texture than the intermediate substance; for, although both had the same appearance after being sawn, the processes bore a polish, (which the other did not,)* and were laminated, like ivory; while the other parts were porous, like the internal structure of common bone.

This led me to examine preparations of the elephant's teeth, in a growing state, preserved in spirit, which explained the mode of growth of these two substances to be different. In these preparations it was found, that the processes of the tooth, which may be called ivory, were all formed upon so many portions of one common pulp, which had its origin in the jaw; and that the intermediate substance, which may be called bone, was formed upon a species of ligament situated immediately under the gum, from which, membranous elongations extended into the spaces between the processes of the tooth.

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* A portion of the jaw itself bore the same degree of polish as the intermediate substance of the tooth.

The cells in the elephant's skull are no part of its common structure; they communicate freely with the cavity of the tympanum, and are therefore appendages to the organ of hearing, which I shall explain more fully on some future occasion.

As this is a subject not favourable for minute description, the annexed drawings will give a more satisfactory idea of what is meant to be described, than can readily be done in words.

In these drawings is represented the ligamentous substance, with its projecting membranes; and the vascular pulps; also the mode in which the ossification takes place in the one, and the formation of the substance of the tooth in the other.

This structure of tooth is not peculiar to the elephant, but common to the teeth of all animals whose food requires to be ground, or much bruised, before it is swallowed.

In the elephant's tooth, from the largeness of its size, the parts are more distinct, and more readily contrasted with each other; but, in other animals, even those of a small size, as the sheep, the different structures are readily detected.

It is singular that this structure should have escaped the accurate investigation of the late Mr. HUNTER; particularly as the formation of the teeth was one of the first objects he employed himself upon; and he continued to pursue it to the end of his life, marking the varieties which occur in different animals.

The cause of his overlooking it was the following: in making preparations of horses' teeth, to show the figured appearance on the grinding surface, he rendered them black by means of fire, which did not affect the enamel, so that the white lines of the enamel were beautifully distinct on the black ground; but the bony part and the substance of the tooth were equally coloured, and had an uniform appearance.

The examination of these preparations led him to believe, that the horse's tooth consisted of only two substances, the tooth itself and the enamel. Under this impression, Mr. HUNTER examined the growing teeth of the horse, and found the pulp rising from the jaw, and the vascular membranes passing down from the gum, into the spaces between the portions of pulp; he was therefore led to conclude, that the pulp was for the formation of the tooth, and that the membranes which came from the gum were for the formation of the enamel.

Having so fully explained, in the elephant's tooth, the real uses of these two parts, it is not necessary to say more in refutation of this opinion, which is published in Mr. HUNTER'S work on the teeth; but, in justice to the correctness of his other observations, I shall subjoin his account of the circumstances under which the enamel of the human teeth is formed, taken from the same work. He says, " the pulps are surrounded " by a membrane, which is not connected with them, except at " their root, or surface of adhesion. This membrane adheres, " by its outer surface, all round the bony cavity in the jaw, " and also to the gum, where it covers the alveoli.

" When the pulp is very young, as in the fœtus of six or " seven months, this membrane is pretty thick and gelatinous. " We can examine it best in a new-born child, and we find it " made up of two lamellæ, an external and an internal: the " external is soft and spongy, without any vessels; the other " is much firmer, and extremely vascular, its vessels coming " from those that are going to the pulp and body of the tooth,

" While the teeth are within the gum, there is always a muci-" laginous fluid, like the synovia in joints, between this mem-" brane and the pulp of the tooth."*

This mucilaginous fluid, I have already asserted, deposits the enamel; which is further confirmed by the following experiments and observations.

The complex tooth of the elephant, being composed of three different structures, each of which has a peculiar process for its formation, led to an inquiry whether the materials themselves were different, or only differently arranged.

To investigate this, Mr. CHARLES HATCHETT, from a zeal to promote the pursuits of science by which he is distinguished, obligingly gave his assistance, and made some experiments, the results of which are as follows.

It is to be understood, that a complete analysis was never intended to be made; as neither Mr. HATCHETT's time admitted of it, nor did it appear necessary for the object of the present inquiry.

Experiment I. Some enamel, rasped into a fine powder, was put into a matrass, and, pure muriatic acid being added, the whole was suffered to remain without the application of heat during one hour; in the course of this time, the enamel was completely dissolved, with a gentle effervescence.

To this solution, some sulphuric acid was gradually added, till all precipitation had ceased: the precipitate was separated by a filter, and was found to be selenite. The filtrated liquor, by evaporation, afforded a small additional quantity of selenite, which was also separated; after which, the liquor, being evapoporated, became thick and viscid. This, when diluted with water, precipitated lime from lime water, in the state of phosphate.

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* Natural History of the Human Teeth, by JOHN HUNTER, p. 86.

To another portion, solution of acetite of lead was added, and caused an immediate precipitation of a white matter, which, when dried and sprinkled on burning charcoal, produced a light and smell like phosphorus; it, moreover, was soluble in nitrous acid, and was thus to be distinguished from muriate or sulphate of lead.

Experiment II. Some of the raspings of enamel were dissolved by digestion in nitric acid, and, when the solution had been diluted and filtrated, it was saturated with carbonate of ammoniac. The precipitate thus produced was collected, and edulcorated in a filter. The small excess of carbonate of ammoniac, in the filtrated liquor, was saturated with acetous acid; after which, the phosphoric acid was precipitated, by solution of acetite of lead. Upon examining the first precipitate, or that produced by the carbonate of ammoniac, it was found (contrary to expectation) that it was still composed of lime, combined with a portion of phosphoric acid, instead of carbonic acid, which might have been supposed.

To effect, therefore, a complete separation of the two ingredients, (lime and phosphoric acid,) acetous acid was poured on the precipitate, by which it was immediately dissolved. The whole of the phosphoric acid was then separated from this solution, by acetite of lead; after which, lest any lead should be present, the liquor was saturated with pure or caustic ammoniac, and the lead was separated by a filter: lastly, the lime which remained dissolved, was precipitated (in the state of carbonate) by carbonate of ammoniac.

The enamel has been supposed, not a phosphate but a carbonate of lime. This error may have arisen from its solubility in acetous acid or distilled vinegar; but the effects of the acetous acid are, in every respect, the same on powdered bone as on the enamel. Consequently, when enamel, or bone, is put into a glass matrass containing acetous acid, placed in a sand bath, the portion which is dissolved, is not (as has been supposed) carbonate but phosphate of lime; for, if to the filtrated solution nitrate or acetite of lead is added, a precipitate is produced, of phosphate of lead, in the same manner as when nitrate or acetite of lead is added to urine.

This mode of treating substances supposed to contain phosphoric acid, as bone, &c. Mr. HATCHETT has found of great utility; because, by this means, he can detect phosphoric acid, when the substance is in too small a quantity to be examined in any other manner.

Similar experiments, on the substance of teeth formed on pulps, and on common bone, afforded similar results.

Mr. HATCHETT considers lime and phosphoric acid to be the essentially constituent principles of these three different structures; and, any difference that is met with, only seems to be that which would constitute species of the same genus, similar to what is found in the mineral kingdom, under lime-stone, marble, and calcareous spar: these differ only by a small change in the proportions of their constituent principles, and by a different arrangement of their integrant particles.

The head of a human thigh bone was found, some years ago, with a thin crust of highly-polished enamel, similar in some respects to that of the teeth, upon a portion of its surface, an inch and half in length, and an inch in breadth; the cartilage having been previously removed by disease. This uncommon appearance, at the time, could not be accounted for; but the forementioned observations, on the formation of the enamel of the teeth, appeared to throw some light upon it; and Mr. Hatchett, at my request, made the following experiment, to determine whether the synovia, in a healthy state, contains phosphate of lime.

960 Grains of synovia, by a gradual evaporation, afforded 21 grains of a substance which resembled dried glue. This, being collected, was put into a small porcelain crucible, which (placed in a larger crucible) was exposed to a red heat, during nearly an hour.

The matter in the porcelain crucible was much reduced in bulk, and appeared like a glazing, thinly spread on those parts of the crucible which had been in contact with it in its former state.

Boiling distilled water was digested on the matter in the crucible, for some time. This water afterwards afforded, with acetite of lead, a copious precipitate of phosphate of lead; but no appearance of lime could be obtained.

On the residuum in the crucible, acetous acid was digested, which was afterwards divided into two portions.

To one of these, solution of acetite of lead was added, and, as before, afforded a plentiful precipitation of phosphate of lead.

To the other portion was added oxalic acid, by which, a small quantity of a precipitate was obtained, which was an oxalate of lime. Phosphate of lime is therefore present in synovia, although but in a small quantity; and as, from these experiments, there is reason to believe, that more phosphoric acid was obtained than was requisite to saturate the lime, it seems probable, that part of it was combined, in the synovia, either with soda or ammoniac; and this accounts for the part dissolved by the distilled water.

M. Margueron, in the *Annales de Chimie*, (Vol. XIV. page 123.) estimates the proportion of water in 288 grains of the synovia of an ox at 232 grains. The other ingredients therefore amount to 56 grains: but, by evaporation, Mr. Hatchett obtained, from 960 grains of synovia, only 21 grains of residuum; which proves that the proportion of water is much greater; for, 56 to 288, is as 1 to 5,14; but, 21 to 960, is as 1 to 45,71.

It is possible, that the proportion of water to the other ingredients may not always be the same. M. Margueron also, probably, estimated the albuminous matter, &c. in a moist state; for, without one of these suppositions, it is impossible to reconcile such a very great difference.

By these experiments of Mr. Hatchett, phosphate of lime was ascertained to be present in the synovia; which, although in a very small quantity in the natural state of that fluid, explains the mode by which the crust of enamel on the head of the thigh bone could be produced, when, by a morbid action of the parts, the quantity of phosphate was preternaturally increased.

A mixture of bony matter with the enamel and the substance of the tooth, is a structure, as has been mentioned, not confined to the elephant: it is common to all truly graminivorous quadrupeds. But the whole number of grinding teeth belonging to each side of the jaw being confined in a case of bone, so as to form one large grinding surface, and the teeth being pushed forward from behind, instead of a second set being formed immediately under the fangs of the first, as in other animals, are peculiarities not met with in any teeth hitherto described, except those of the elephant.

These peculiarities have, however, been ascertained, in the course of the present inquiry, to belong to the *Sus Æthiopicus;* a skull of which, with the teeth, is preserved in Mr. HUNTER's collection. The particular species to which it belonged was determined, by its exact similarity to a skull, without the grinding teeth, in the British Museum, marked, in Dr. SOLANDER's hand-writing, *Sus Æthiopicus*, from Guinea.

As the grinding teeth of this animal have not been before noticed, figures of the head and teeth are annexed, (Tab. XVIII. and XIX.) and, as it has been ascertained by Dr. SOLANDER to come from Guinea, there is reason to hope so curious a species of the hog will attract the notice of naturalists, and be the means of perfect specimens being introduced into this country.

From the appearance of the teeth in the perfect skull, the animal had probably arrived at its full growth, and only one grinder remained on each side of the jaw, consisting of seven different processes, cased with bone, similar to those of the elephant. The grinding surface of those processes which had their points worn down sufficiently to show a transverse section, exposed three oval portions of tooth, surrounded by enamel, inclosed in bone; which is more like the tooth of the African elephant than the Asiatic, and makes another variety of form of these processes.

The tusks of the *Sus Æthiopicus* are uncommonly large, and in their structure resemble those of the elephant.

The skull was shown to Sir JOSEPH BANKS, whose readiness to forward the labours of those who engage in the pursuits of science, by liberally communicating to them his own knowledge of the subjects connected with their inquiries, is sufficiently known to the members of this learned Society. He identified the species of the genus to which the skull belonged, in the manner above mentioned; and, by an accurate search among the skulls of animals deposited in the British Museum, discovered a small head in a dried state, which, when properly macerated and cleaned, proved to be that of a young *Sus Æthiopicus*, whose teeth were in a growing state, and enable me to explain all the necessary circumstances respecting this curious mode of dentition.

The grinding teeth, in this young head, are distinct from each other, and four in number, on each side of the jaw.

That which is most anterior is the smallest, and has a grinding surface only equal in extent to that of one of the processes contained in the large tooth of the full grown animal: the second has a grinding surface equal to that of two such processes: the third is still larger, its surface being equal to that of three processes.

These three teeth, in their general appearances, resemble those of the common hog; they have also the same kind of fangs; their only peculiarity is, the enamel being intermixed with the substance of the tooth, but without any bony matter surrounding it.

The fourth or last tooth is very different from the others, and exactly resembles that found in the large head, only that this is in a growing state. It is composed of seven processes, united together; these are in different stages of growth, fitting them to come forward in succession, similar to those of the elephant. The two first have their grinding surface worn smooth: the points of the two next have recently cut the gum; and the other three are still concealed in the jaw, not being completely formed; of the last of these, the first rudiments only are to be seen.

This large tooth, (which may be considered to be a second set of teeth,) as the concealed processes enlarge, advances forwards, pushing the other teeth before it: the most anterior of these, as soon as its body is worn away, has its fangs removed by absorption, and drops out: the same thing takes place with the second and third; and, in this way, room is made for the large one to supply the place of all the others.

The mode in which they succeed one another, is illustrated by the annexed drawing of a side view of the jaw, Tab. XIX. in which the fangs of the different teeth are exposed; and the body of the third tooth, having been moved forward, as the last increased in size, is distinctly seen.

These peculiarities in the teeth of the *Sus Æthiopicus*, led to the examination of the teeth of the other species of the same genus; all of which appear to resemble the human grinders, only that the last in the jaw has a broader grinding surface than the rest, which is common to most quadrupeds. It is worthy of remark, that the number in each side of the jaw in the common hog is seven; in the Pecary, six; in the Babyroussa, five; and in the *Sus Æthiopicus*, till a certain age, four.

It is curious, that one species of a genus should differ so widely from all the others, in respect to its teeth; and should be allied to the elephant in the structure of its tusks, the mode of formation of the grinding teeth, and the manner in which they succeed one another. From these circumstances it appears, that the *Sus Æthiopicus*, in a natural state, is supplied with a different kind of food from that of other hogs, and is an animal of greater longevity.

Upon comparing the internal structure of the elephant's tooth with that of the horse, cow, and sheep, it was found, that they were similar in having an intermixture of bone with the substance of the tooth, but that they differed materially from one another in the proportions and situations of the bony portions. Each of these animals having the grinding surface of their teeth adapted for particular kinds of food, the parts composing that surface are variously combined, so as to answer the purpose for which the teeth are intended. In all of them, the mode of growth is the same; the substance of the tooth is first formed, and the bony part is afterwards adapted to the irregularities of that surface.

In the horse's grinding teeth, the processes are two in number; and, in an early stage of their growth, they appear, as well as those of the elephant, to be separate teeth: they differ, however, extremely in their shape, forming irregular cylindrical tubes, the central part of which is filled up by the projecting membranes from the gums, that are to be changed for bone. This division of the tooth into two parts, is very distinct in the shedding teeth, but not in the second set or permanent teeth.

These two portions of bone in the middle of the tooth, have frequently a hole in them, (probably the passage of a blood-vessel, never completely filled up,) and the food getting into it, as the tooth is worn down, considerably increases its size. Besides which, there is a portion of bony substance surrounding a great part of the outside of the tooth.

In the cow's grinding teeth, there are two portions of bony substance in the middle of the tooth, as in the horse, in shape of crescents, and a very small portion in the hollows on the outside of the circumference of the tooth; but none on the projecting parts.

In the grinding teeth of the sheep, the middle portions of bone are similar to those of the cow, but on a much smaller scale; there is no portion of bone on the outside of the tooth.

It is not to be wondered at, that there is so great a variety in the grinding surfaces of the teeth of different genera of graminivorous quadrupeds, each, no doubt, adapted to the kind of food they are in a state of nature destined to live upon, since there is even a variation between the teeth of the African and Asiatic elephants. In the African elephant, the processes of which the tooth is composed are not flattened ovals, as they have been described in the Asiatic, but are in the form of an oblong square or parallelopipedon, so that, in the middle line of the tooth, the processes are in contact with each other, although at no other part; by this means, the middle line of the tooth is the hardest; the whole surface therefore does not wear regularly, as in the Asiatic elephant, but with a ridge in the middle.

As a description of the grinding surface of the teeth of the African elephant, the horse, cow, and sheep, would be very tedious, to make it at all intelligible, and as the surfaces of the teeth can be very distinctly represented in a drawing, I have preferred that mode of showing their comparative structure. See Tab. XVI. and XX.

Having, by the foregoing observations, established a well marked characteristic distinction between the teeth of truly carnivorous and truly graminivorous quadrupeds, I was desirous of knowing how far this general rule applied to quadrupeds at large, and, if it did not, in what animals the teeth were differently formed.

The teeth of the hippopotamus and rhinoceros are found to differ in their structure from those above described, partaking in some measure of the properties of both, and forming two very curious links in the chain of regular gradation between the one and the other.

The grinding teeth of the hippopotamus are made up of the substance of the tooth and enamel only, having no portion of bone mixed with the other parts; but, what is I believe peculiar to them, the enamel pervades the substance of the tooth to a considerable depth, so as to be intermixed with it. This will be better understood by referring to the figures, (Tab. XX.) which represent the appearance of the enamel in different sections of the tooth.

The grinding teeth of the rhinoceros have a peculiarity of a very different kind: they also are only composed of the substance of the tooth and enamel; but the tooth is so formed as nearly to surround a middle space, which, were it filled up with bone, would make a truly graminivorous tooth, not unlike those above described. This middle space is left open, and becomes filled up with the masticated food, which falls into it, and cannot afterwards be readily removed; so that the grinding surface will be always kept irregular, and in a still greater degree than in any of the other teeth which have been described. The particular form of this tooth is represented in Tab. XXI.

It is highly probable that there are many other varieties in the structure of the grinding teeth of quadrupeds, but these will be sufficient to illustrate the general principles upon which such varieties depend.

## EXPLANATION OF THE PLATES.

### Tab. XIII.

A longitudinal section of a grinding tooth of the Asiatic elephant, in a growing state. The tooth had been previously steeped in the muriatic acid, to render it soft, that it might be divided with less injury to the tender parts.

The different processes which compose the tooth are distinctly seen, but the enamel which surrounded them was dissolved in the acid; so that there is a vacant space, between them and the bony substance in which they appear to be imbedded. The bony substance, which is completely formed along the upper surface of the tooth, is hard and compact; but the projecting portions, near their termination, are not yet ossified, but are in the state of a membrane.

The cavity of the body of the tooth is filled with a vascular pulp, portions of which pass up into the processes, for their formation: these are conical in their shape; so that the processes of the tooth formed upon them, as they become longer, have their sides separated more and more, till at last the lower edge comes in contact with, and unites to, that of the neighbouring processes, connecting them together, and forming the cavity of the tooth, to which the fangs are afterwards added.

*aaaa.* The pulp on which the tooth is formed.

*bbbb.* The processes of the tooth, formed upon the pulp.

*cccc.* The intermediate bony substance between the processes.

## Tab. XIV.

The rudiments of the bony substance which makes a portion of the elephant's tooth, preserved in spirit, before ossification had begun to take place.

It is represented here, to show that it has an entirely different texture from the pulp on which the tooth itself is formed, and resembles the membranes in which ossifications have their origin, for the formation of the flat bones of animal bodies.

In the more solid parts it resembles ligament; but, where it is thin, as towards the terminations of the projecting portions, it is exactly similar to membrane.

## Tab. XV.

A longitudinal section of the elephant's tooth, highly polished, to show more perfectly the different textures of which it is made up. These, being of different degrees of hardness, bear a very different polish, which renders them distinct from one another: they are also distinguishable, by having different arrangements of the parts which form them. In the enamel, the texture is fibrous, and the direction of the fibres is transverse. In the processes, the texture is laminated, and the direction of the laminæ is longitudinal. In the bony part, there are no distinct fibres, nor laminæ.

In this tooth, a small portion only is completely formed, the rest being still in a growing state. The fangs are not yet added: there is, however, the origin of one of them, at that end of the tooth which is most perfect, and which has had a small portion of its grinding surface in use.

### Tab. XVI.

The grinding surface of the African elephant's tooth, to show that the processes of which it is made up, are of a different shape from those which compose the tooth of the Asiatic elephant.

To see the true shape of each process, it is necessary that the points should be worn down to some depth, which, in the present specimen, is only the case with the four largest; the other three, which are less worn, show the different thicknesses of the processes, nearer their termination on the upper surface of the tooth.

### Tab. XVII.

A longitudinal section of a portion of the African elephant's tooth, highly polished, to show the relative situation of the substance of the tooth, the enamel, and the bone, of which it is composed. The cavity of the tooth, and the fang, are also exposed.

A thin narrow slip of the bony part only is seen between the processes, in consequence of the processes, in the central line of the tooth, coming nearly in contact with each other.

### Tab. XVIII.

A side view of the skull of the *Sus Æthiopicus*, (half the natural size,) to show the situation and appearance of the large grinder, and the remains of the alveoli belonging to the fangs of the preceding one.

## Tab. XIX.

Fig. 1. A side view of the skull of the young *Sus Æthiopicus*, to show the mode in which the grinders come forward, as the large one increases in size.

Fig. 2. A side view of the full grown grinder tooth.

Fig. 3. A transverse section, polished, to show the mixture of bone with the enamel and substance of the tooth, and the particular appearance of the processes.

## Tab. XX.

Fig. 1. A transverse section of the horse's grinding tooth, polished, to show the relative situation of the bony part, with respect to the enamel and substance of the tooth. Two portions of it are inclosed by enamel, and a third portion is on the outside of the tooth altogether.

The two holes were probably to give passage to arteries, and were enlarged by the food falling into them, and wearing away their sides.

Fig. 2. A similar section of a grinding tooth of the cow. The shape of the bony portions inclosed by the enamel is different, and there is a smaller portion of bone on the outside of the tooth.

Fig. 3. A similar section of a grinding tooth of the sheep. The portions of bone inclosed by the enamel are very small, and there is none on the outer side of the tooth.

Fig. 4, 5, and 6. Are three sections of a grinding tooth of the hippopotamus, to show that there are portions of enamel intermixed with the substance of the tooth, but no bone whatever.

Fig. 4. A transverse section, polished, similar to those of the other teeth.

Fig. 5. A view of the grinding surface of a shedding tooth, to show the uncommon appearance of its surface, before it is worn by use. The fangs have been absorbed.

Fig. 6. A longitudinal section of a full grown tooth, to show the plate of enamel passing down the centre: on one side, the enamel passes lower down than on the other.

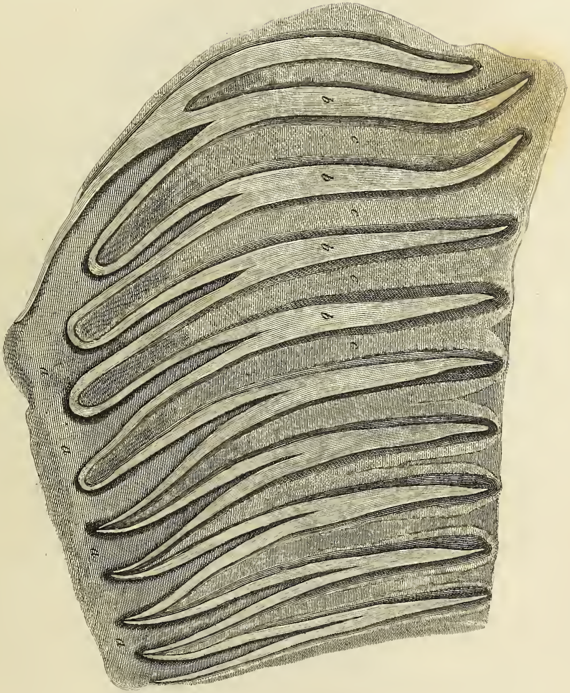
## Tab. XXI.

This plate contains three views of a grinding tooth of the rhinoceros, to show its shape and internal structure.

Fig. 1. A view of the grinding surface, taken obliquely, seen from the side of the tooth next the mouth, to show the middle space, which is hollow, between the two projecting walls of the tooth.

Fig. 2. A front or external view of the tooth, showing its shape and size, also the number of its fangs.

Fig. 3. A transverse section of the tooth, polished, to show the peculiar shape of the middle space, and the irregular projecting portions of the tooth, in that direction; also the mode in which the enamel surrounds every where the substance of the tooth.



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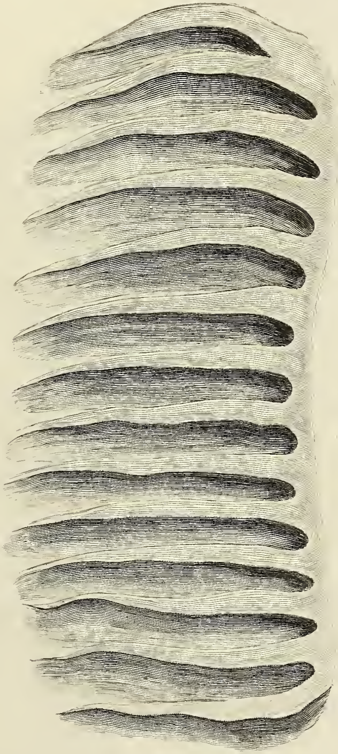

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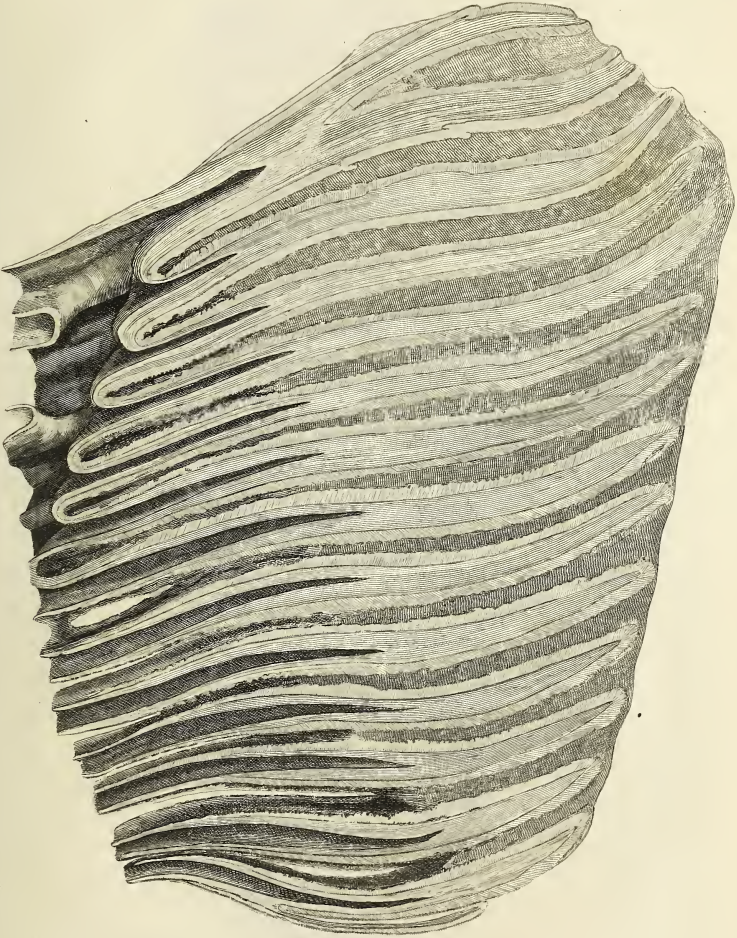

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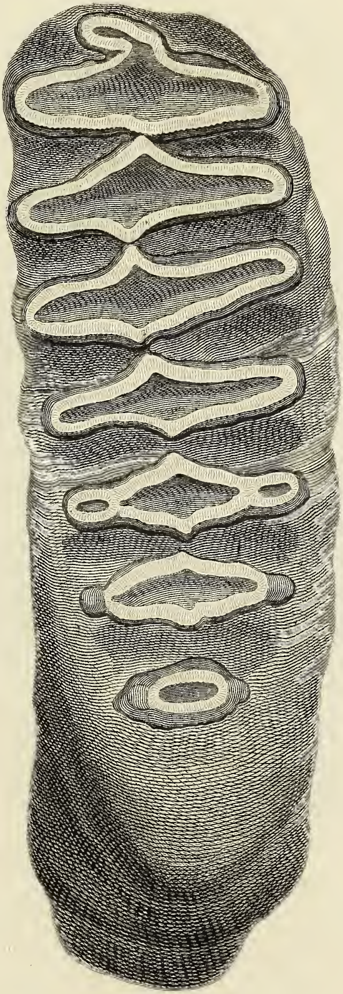

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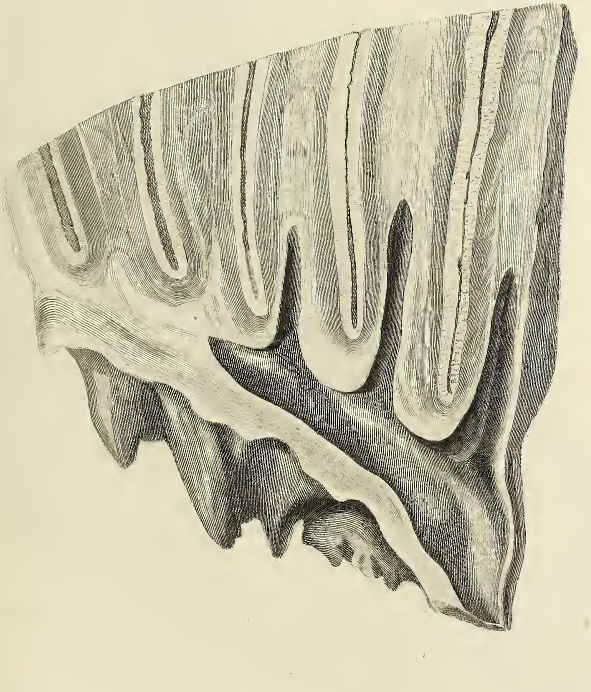

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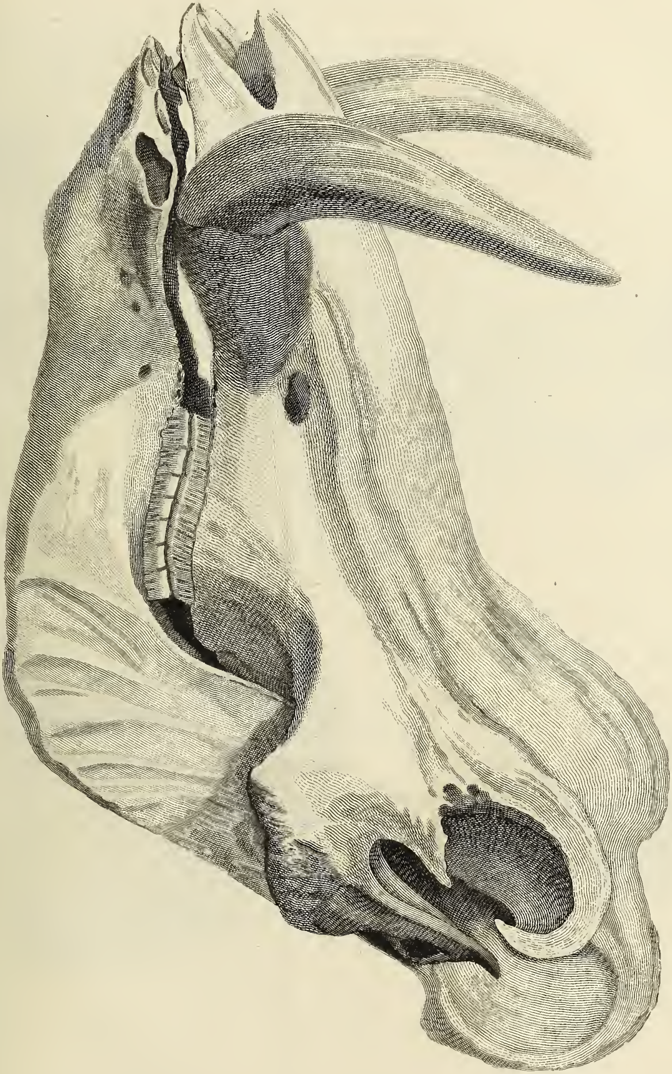

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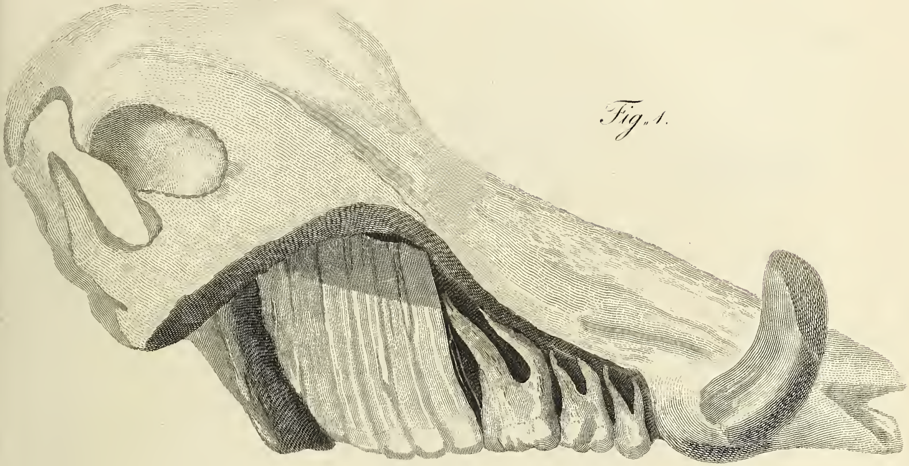
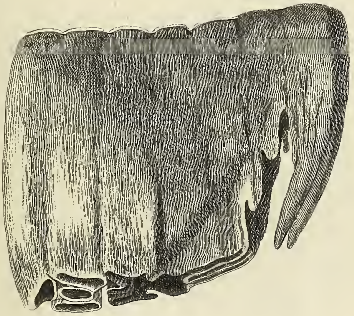


Fig. 1.

Fig. 2.



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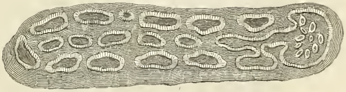

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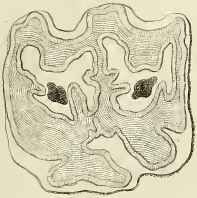
Fig. 1.



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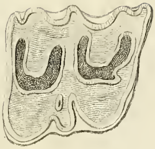

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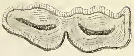


Fig. 4.

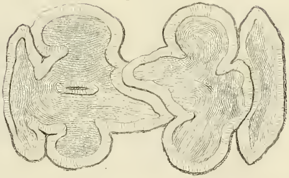
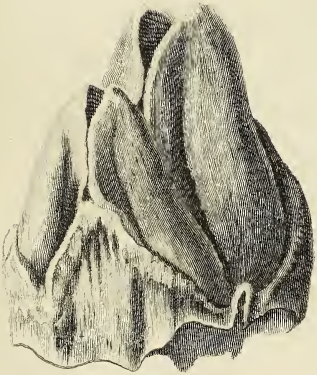


Fig. 6.

Fig. 5.



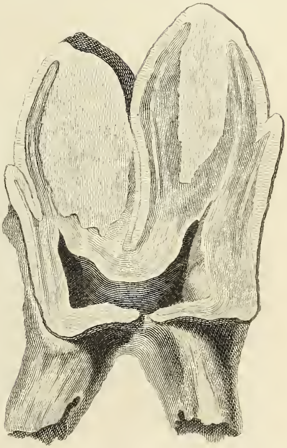


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Fig. 1.

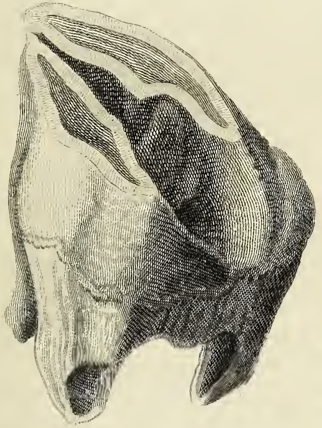


Fig. 2.

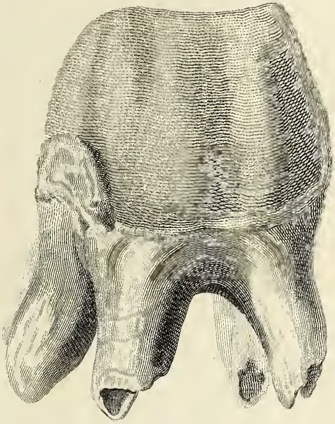
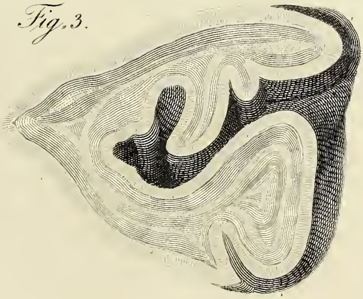


Fig. 3.



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BRITISH MUSEUM NATURAL HISTORY

# XV. *Experiments to determine the Quantity of tanning Principle and gallic Acid contained in the Bark of various Trees. By* George Biggin, *Esq. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read June 6, 1799.

THE bark of trees contains the astringent principle, called gallic acid, and also that principle which has a peculiar affinity to the matter of skin, and which, from the use to which it is applied, is called the tanning principle. But, in the present mode of tanning, bark is applied in *mass* to the skins; consequently, *both* principles are applied. It remains for examination, whether both principles are useful in the process of tanning; for, if they are not both useful, probably *one* is detrimental.

To a nobleman, whose zeal on every occasion by which the sciences or arts may receive illustration or improvement is eminently conspicuous, and to whose public energy, as well as private friendship, I feel myself much indebted, to his Grace the Duke of BEDFORD, I owe the means of prosecuting some experiments on this subject. His Grace, by collecting a variety of barks, at Woburn, gave me an opportunity of making some experiments to ascertain the quantity of tanning principle and gallic acid each bark contained. For that purpose, I made use of the following methods, according to the principles laid down by M. SEGUIN.

By dissolving an ounce of common glue in two pounds of boiling water, I procured a mucilaginous liquor, which, as it contains the matter of skin in solution, is a test for the tanning principle. By a saturated solution of sulphate of iron, I obtained a test for the gallic acid.

I then took one pound of the bark I meant to try, ground as for the use of tanners, and divided it into five parts, each part being put into an earthen vessel. To one part of this bark, I added two pounds of water, and infused them for one hour. Thus I procured an infusion of bark, which I poured on the second part of the bark, and this strengthened infusion again on the third part, and so on, to the fifth. But, as a certain portion of the infusion will remain attached to the *wood* of the bark, after the infusion is poured or drawn off, I added a third pound of water to the first part, and then followed up the infusion on the several parts, till the three pounds of water, or so much of them as could be separated from the bark, were united in the fifth vessel; from which I generally obtained about one pint of strong infusion of bark.*

To a certain quantity of this infusion, I added a given measure of the solution of glue; which formed an immediate precipitate, that may be separated from the infusion by filtering paper. When dried, it is a substance formed by the chemical union of the matter of skin with the tanning principle, and is, in fact, a powder of leather. By saturating the infusion with the solution of glue, the whole of the tanning principle may be separated by precipitation.

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* The specific gravity of this infusion was ascertained by an hydrometer whose gradations are inverse to those of a spirit hydrometer.

### FOR THE GALLIC ACID.

To the pound of bark left in the earthen vessels, and already deprived of its tanning principle by these *quick* infusions, I added a *given* quantity of water, to procure a strong infusion of the gallic acid, which requires a longer time, (say 48 hours.) This infusion, when obtained pure,* affords little signs of the presence of the tanning principle, when tried by the test of the solution of glue; but, with the solution of sulphate of iron, it gives a strong black colour, (the common black dye,) which differs in density, according to the quality of the bark: this may be further proved, by boiling a skain of worsted in the dye, by which the gradations of colour will be very perceptibly demonstrated.

Having thus obtained a point of comparison; by making a similar infusion, under similar circumstances, of any bark, or vegetable substance, and paying strict attention to the specific gravity of the infusion, the quantity of precipitate of leather, and the density of colour produced by given quantities of one or the other test, the result will be, a comparative statement of the respective powers of any bark, or vegetable substance. This comparative statement, I conceive to be sufficient for all commercial purposes.

As oak bark is the usual substance employed in the trade of tanning, if a quantity of tanning principle is found to be contained in any other bark or vegetable, the commercial utility of that bark or vegetable may be determined, by comparing its quantity of tanning principle and price with those of oak bark.

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* It is hardly possible, from the intimate connection of the two principles, to separate them entirely by infusion: in the infusion of tanning principle, there will always exist a little gallic acid; and, in an infusion of gallic acid, a little tanning principle will commonly be present, unless the infusion of gallic acid is very weak, and procured by a third or fourth watering.

For an accurate chemical analysis, I have tried a variety of acids, and simple and compound affinities; and, having pursued the above experiments, at the same time that I was employed on some in dying, I found the muriate of tin (the method of using which is described by Mr. PROUST in the *Annales de Chimie*) very convenient. A solution of it, being added to the infusion of bark, forms a precipitate with the tanning principle, leaving the gallic acid suspended: the precipitate is of a fawn colour, and is composed of tanning principle and oxidated tin.

By these means, I have been enabled to form a comparative scale of barks; which, however, I do not produce as accurate. Oak bark, in its present state, as procured for commercial purposes, differs very much in quality, from accidental circumstances: the season of the year in which it is collected occasions a still more important difference, consequently, the scale now produced must be very imperfect; but I am of opinion, that by the pursuits of scientific men who may be inclined to investigate this subject more fully, a very accurate scale may hereafter be formed.

In the following scale, I have taken Sumach as the most powerful in the comparative statement; leaving, however, a few degrees, for a *supposed maximum of tanning principle,* which I reckon 20.

## SCALE OF BARKS.

| Bark of | Gallic acid, by colour. | Tanning principle, by hydrometer. | Tanning principle, (in grains,) from half a pint of infusion and an ounce of solution of glue. |
|---|---|---|---|
| Elm* - - - - - | 7 | 2,1 | 28 |
| Oak, cut in the winter - - | 8 | 2,1 | 30 |
| Horse chesnut - - - | 6 | 2,2 | 30 |
| Beech - - - - - | 7 | 2,4 | 31 |
| Willow (boughs) - - - | 8 | 2,4 | 31 |
| Elder - - - - - | 4 | 3,0 | 41 |
| Plum-tree - - - - | 8 | 4,0 | 58 |
| Willow (trunk) - - - | 9 | 4,0 | 52 |
| Sycamore - - - - | 6 | 4,1 | 53 |
| Birch - - - - - | 4 | 4,1 | 54 |
| Cherry-tree - - - - | 8 | 4,2 | 59 |
| Sallow - - - - - | 8 | 4,6 | 59 |
| Mountain ash - - - | 8 | 4,7 | 60 |
| Poplar - - - - - | 8 | 6,0 | 76 |
| Hazel - - - - | 9 | 6,3 | 79 |
| Ash - - - - - | 10 | 6,6 | 82 |
| Spanish chesnut - - - | 10 | 9,0 | 98 |
| Smooth oak - - - - | 10 | 9,2 | 104 |
| Oak, cut in spring - - | 10 | 9,6 | 108 |
| Huntingdon or Leicester willow | 10 | 10,1 | 109 |
| Sumach - - - - | 14 | 16,2 | 158 |

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* The infusion of elm was so loaded with mucilage, that it was with difficulty I could separate the tanning principle, or try the specific gravity.

It is to be observed, that the barks do not keep any respective proportion in the quantity of gallic acid and tanning principle contained in each; which is an evidence of the distinctness of principle, and may perhaps open a new field for saving oak-bark in dying, as the willows, sallow, ash, and others, produce a very fine black. It is also worthy of observation, that the quantities of gallic acid and tanning principle do not differ in *equal* proportions, between the winter and spring felled oaks. This fact may lead to the discrimination of the proper time for cutting; which is, probably, when the sap has completely filled and dilated that part of the vegetable intended for use. This will make a difference in the season of cutting oak, elm, and other trees, shrubs, &c. Leaves should be taken when arrived at their full size, and then dried under cover; for, as the tanning principle is so soluble, and the substance that contains it so thin, (in a leaf,) the dew alone might dissolve it.

Finally, as the gallic acid does not seem to combine with the matter of skin, and as its astringency will corrugate the surface, we may, I think, conclude, that its presence in tanning is not only useless, but detrimental.

# XVI. *Essay on the Resolution of Algebraic Equations: attempting to distinguish particularly, the real Principle of every Method, and the true Causes of the Limitations to which it is subject. By* Giffin Wilson, *Esq. Communicated by* Edward Whitaker Gray, *M. D. Sec. R. S.*

Read June 6, 1799.

## INTRODUCTION.

1. THE *practical* management of algebraic equations, as far as respects the solution of problems depending upon them, is well understood; but their *general theory*, being considered as an abstruse and purely speculative subject, is no where, that I have seen, so fully analysed, as with all the assistance to be derived from the application of the principles of combination, it appears to me it might be.

2. The difficulties under which the higher branches of algebra still labour are generally known. No degree of equations beyond the second, is yet perfectly resolved: cubics present frequently an irreducible case: biquadratics have, by several methods, been reduced to cubics; but no formula exhibiting to the eye the actual resolution of a biquadratic has yet appeared: and, for the fifth degree, and all upwards, not even a clue which promises a general resolution has been struck out, by the continued labour and ingenuity of mathematicians for several centuries.

3. This failure in the chain, beginning at the third degree,

and its breaking off entirely after the fourth, have been very puzzling and mortifying circumstances to the cultivators of algebra. Having in the first degrees proceeded upon apparently very general principles, and made a seeming progress towards a general resolution of equations, it is provoking to find it suddenly interrupted, not to be resumed *by any contrivance.* Various causes have been assigned for so remarkable a difficulty; but the generality of those causes, as commonly given, do not reach the principle. It has been usual for operators, when they found their methods fail, to look back till they could detect some inconsistence or impossibility in their work, and to suppose the difficulty explained, by pointing out the period at which such an error is made. The power and richness of the algebraic calculus affords numerous ways of compassing the same thing; and, as *all* of them fail when applied to this object, there is necessarily a point in *every one of them,* at *which* some inconsistence or impossibility is introduced: thence, a number of different causes may be imagined. In Dr. WARING's *Meditationes Algebraicæ,* (p. 182.) may be seen several concurrent reasons assigned, why the methods there shewn, and Dr. WARING's own, (undoubtedly the most general of any of them, since it proceeds upon one principle to the fifth degree,) cannot apply further: but, all reasons drawn from the data of any particular method, (like that commonly given for the imperfection in CARDAN's Rule, which I shall examine hereafter,) though very just in themselves, cannot be conclusive: they indisputably shew, why the precise method to which they respectively apply must fail; but that does not exclude the expectation that some other, founded upon different principles, may succeed. The question therefore recurs: Is there not some paramount fundamental reason for this general failure? If there can be shewn to be any thing *in the nature of abstract quantity*, which governs the several orders of quantities from which equations are framed, and leads *directly* to the distinctions and limitations practice discovers, *that* will reach the difficulty at its source, and afford the satisfaction desired.

4. I think, that by turning the course of our inquiry rather to examine how we come to succeed at all, in resolving *any* degree of equations, than why our success is so limited, the *true* principle upon which their resolution must depend will appear; and with what probability, and by what means, (if possible,) we may expect to render our methods more perfect. With this idea, I shall take a concise view of the nature and resolution of equations in general; pointing out the common difficulty, and by what circumstances that difficulty is, in certain cases, lessened or removed; confining myself always to the *principle* of each step, *and a strict analysis of the result*, avoiding all detail of mere operation; and, without pretending to much novelty upon a subject already so beaten, I persuade myself, such an investigation will lead to some conclusions *which have not been remarked*, and which are both curious and important.

## CHAP. I.

### *Of the Resolution of Equations in general.*

5. EQUATIONS, in that part of algebra which treats of their general resolution, are usually considered to be reduced to *one general form*, for the greater convenience of comparing them,

*i. e.* to their lowest rational dimension, with unity always for the coefficient of the highest power of the unknown quantity; in which state, every simple equation is already resolved. The resolution of all other degrees, is the finding the simple equations of which they are compounded: but, to do this in a general manner, it is evident we must seek, instead of the particular equations themselves directly, a general expression representing them all; which general expression is called the formula of resolution, such as, the common quadratic resolution, or that given for cubics by Cardan's Rule.

6. These formulæ, properly speaking, are rather the *reversion* of an equation, than the resolution of it: for, although the unknown quantity be evolved or reduced to a simple dimension, the known parts are necessarily involved or affected with a surd at least as high as the dimension of the equation, in order to exhibit the proper number of correspondent values belonging to the unknown quantity in an equation of that degree. Thus, the equation $(x^2 - px + q = o)$ and its common resolution $\left(x = \frac{p \pm \sqrt{p^2 - 4q}}{2}\right)$ *are both the same quadratic;* only, under the first form, the unknown quantity, being of the dimension of the second degree, has *two* values; whereas, in the second form, it has only *one*, and the double value is transferred, by the quadratic surd, to the known parts on the opposite side of the equation. Thus also, the equation $(x^3 - qx + r = o)$ and the Cardanic formula belonging to it $\left(x = \sqrt[3]{-\frac{r}{2} + \sqrt{\frac{r^2}{4} - \frac{q^3}{27}}} + \sqrt[3]{-\frac{r}{2} - \sqrt{\frac{r^2}{4} - \frac{q^3}{27}}}\right)$ are, in the same manner, *the same cubic merely reverted.* But, as equations are usually denominated from the dimension of the unknown quantity, these reso-

lutions are commonly deemed simple equations: they may in this view be defined to be, *the simple equations* that the original quadratic, cubic, or other higher given equation, *contained in power*, since they express the nature and form of a *quantity which*, by involution or reverting the operation, re-produces it; as the root of any power, being reinvolved, returns to the power from which it was extracted. This fixed and visible connection between the equation and the general formula for its roots, throws a beauty and elegance into the method of *pure algebraic resolution*, which none of the others, such as the method of divisors, and all the contrivances for approximation, can pretend to. For, when by any of those methods we have obtained one or more separate roots, the relation to the original equation is no longer perceivable; but *here* the chain is perfect. The equation leads to the resolution: the resolution embraces at once all the correspondent roots; and, when reinvolved, proves the operation, by reproducing the original equation. Thus, for example, if $(x^2 - 5x + 4 = o)$, and it be perceived, or found by any conjectural method, that unity is one of the roots of that equation, there is no discernible connection between the simple equation expressing $(x = 1)$ and the original equation; *no* transformation of *one* will produce the *other*. This latter equation $(x = 1)$, though truly expressing a numeral root of the former, is no more a resolution of it than of the equations $(x^2 - 6x + 5 = o)$, $(x^2 - 7x + 6 = o)$, or any other of the infinite number of equations of which unity is a root; whereas, the algebraic resolution of $(x^2 - 5x + 4 = o)$ *viz.* $\left(x = \frac{5 \pm \sqrt{25 - 16}}{2}\right)$, which equally expresses $(1)$, and $(4)$ the other root, needs only to be cleared of its radical, to shew itself but another form of the same equation; and gives ($x^2 - 5x + 4 = 0$) as at first.

7. This view of the algebraic resolution of an equation shews, that it does not so much aim at giving us the roots themselves, as the basis or common principle of their artificial combination in the equation to which it applies; pointing out some form of a perfect power, of which they may be conceived to be the correspondent natural roots. From which it follows, that if the transformation required to be made in the given equation be possible, or such as can *really* be effected, the resolution will be real; for every real power has some real root: but that if, on the contrary, the power into which the equation is conceived to be transformed be merely imaginary, the resolution must be so too; for all the roots of an imaginary power are themselves imaginary. It doth not therefore depend upon the nature of the roots of the equation *themselves,* but on the form which the equation must assume to become a perfect power, to determine, whether the resolution be real or imaginary: so that the nature of the resolution, and *that of the roots* of an equation may be very different, as we know is frequently the case; particularly in the resolution of cubic equations by CARDAN's rule, where, when the roots are real, the resolution is almost always imaginary. This has seemed to surprize and perplex some writers very much, who have treated it as at best a paradox, if not a contradiction,* but surely without cause; for, as the formula affects only to be an ideal representation of the mechanism or structure of a perfect power

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* Vide PLAYFAIR on the Arith. of Impossibles, Phil. Trans. 1778, p. 318; Dr. HUTTON on Cubic Equations, ditto, 1780, page 387; and Mr. BARON MASERES, *Script. Logarith.* Vol. II. p. 456.

answering to the given affected equation, it may be expected to be clear or complicated, real or imaginary, *not* as the roots themselves are simple and real, but as the principle of their union, *of which only it is truly the index*, is near or remote: it merely shews the central point of their combination, which, like the centre of gravity, suspension, or any other power, may not actually exist in *any* of the bodies whose motions it governs, but in some imaginary point without, and remote from them all. Had the nature of the algebraic resolution of an equation been considered in this light, and the forms to which they are proposed to be reduced, been compared with the original forms of the roots in the given equation, no surprize or appearance of paradox could have arisen in the matter; but it must have been clearly perceivable, what cases would admit of *real*, and what only of *imaginary* resolutions, as will be shewn hereafter.

I have dwelt the longer upon the nature of the *algebraic resolution* of an equation, because it is a very curious subject, about which many errors and inconsistencies have been fallen into, though hardly any direct examination of it is to be found in any of our books. It is the sole method of obtaining a complete general answer to any problem. It makes algebra consistent with itself, and sufficient to solve its own difficulties, without foreign aid, (from series or other branches;) and, in *all* cases where any *general ulterior use* is to be made of the resolution of an equation, is the only method that avails at all.

8. In order to obtain this general resolution, the common methods have been, (without considering the nature of the roots,) to attempt some universal reduction in the *forms* of equations; as,

1st. The destroying their intermediate terms, and converting them into pure powers. Or,

2dly. The discovering some constant complement which will always raise them to the *nearest* perfect power. In both which cases, the resolution will afterwards be nothing more than simple extraction of the proper root. Or,

3dly. The assuming some convenient formula with indeterminate coefficients; and, by assigning their values properly, adapting it to every case.

It would be going to too great a length, to give distinct examples here, of the application of these methods. Numerous instances of *each* of them are given in the common books of algebra, which usually treat them as separate and distinct from each other; but the fact is, *they are all in truth the same.* Whoever tries them separately, will find, however variously they seem to set out, they lead precisely to the same conclusions, and fail precisely in the same points. A quadratic, whether resolved by completing the square, or by expunging the second term: a cubic, whether resolved by CARDAN's rule, or by completing the cube, or by assuming a resolution, as suggested in Dr. WARING's *Meditationes Algebraicæ*, (p. 179, 180.) present the *same* formula of resolution, and the *same* limitations and irreducible cases. And the reason is easily found. To complete the requisite power, (according to the index of the equation,) or to destroy the intermediate terms, occasions an alteration in *just the same number of terms;* it is only the particular relation they are required to bear to each other that is varied. In the *one* case, they are all to be equal, (or equal to nothing;) in the other, to correspond respectively with the known law of the binomial theorem, which gives the unciæ of a regular power. *Both* depend upon the practicability of a more general problem, of which they are but specific cases; *viz.* the problem "*to give the coefficients of an equation any general determinate relation.*" If *that* were practicable, and it were possible to mould them *so* as to establish a general relation between them, (or any required number of them,) it is easy to perceive, that the particular relation must be a secondary consideration; and that, wherever the *same number* of terms are to be acted upon, the same means that might make them equal, might give them any other proportion at pleasure.

9. However, of all these methods, and any other of the kind, it is to be observed, that the principle is demonstrably a false assumption. For, if it be once admitted that the construction of equations, and the laws of the successive coefficients received ever since VIETA's time, be true; or that all equations are formed invariably in the same manner, from the continual multiplication of the simple equations of their roots, which experience confirms without any exception;* it follows, that the nature of the roots must infallibly govern that of the equation derived from them; that the same form of equation can *only* be produced by the same forms of roots; and therefore, before *all sorts* of equations can be made into pure or perfect powers, or be given any other general shape, it must be shewn, that *all quantities are capable of taking the forms required to produce equations of that sort*, which will presently be seen to be impossible. If those who have lost their time and labour in vain

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* Some algebraists, affecting to reject the use of negative quantities, have been compelled to dispute the generally received theory of the construction of equations; but they have not been able to suggest any other.

endeavours to improve these general methods, had, instead of involving themselves in a labyrinth of substitution and process, upon the chance of some means of simplification presenting itself, considered beforehand the probability of success, the imperfection of CARDAN's rule would never have appeared a paradox, nor the interruption of all further progress by it have given room for surprise. They must have seen, that no equation beyond a quadratic can admit of a *real* extinction of its intermediate terms. In the general equation $(x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4}$ &c. $= 0)$, $(p)$ being the sum of the roots, and $(q)$ the sum of their combinations in pairs, by Sir I. NEWTON's theorem for finding the sums of the powers of the roots, $(p^2 - 2q)$ will be the sum of their squares; and therefore, if both $(p)$ and $(q)$ vanish, the sum of the squares of the roots must vanish also; which can *never* happen with real quantities. Besides this, in attempting to destroy many intermediate terms at once, we know by experience, the equations that become incidentally necessary to be solved, rise to a much higher dimension than the given equation; so that our labour, in this respect, defeats itself.

10. Nor will these difficulties be avoided, if we abandon the idea of a general resolution, and attempt to work out the roots separately: although the number of coefficients is always sufficient to afford a distinct equation to *each* root, and therefore, by the common principles of indeterminate equations, will clearly determine them all; and would also find them, if the equations afforded by the coefficients were all of the same degree; but they rise successively, and, from the drawing them together, in order to expunge the several unknown quantities, the index of the reducing equation *increases* so as to defeat the

operation. To shew this, let us recur to the general equation before given $(x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4} = 0)$; suppose its $(n)$ roots to be represented by ($a, b, c, d,$ &c. $n$*), then, by the construction of equations, we have $(n)$ distinct equations from the several coefficients in succession; *viz.*

$a + b + c + d$ &c. . . . . $+ n$ - - in number $(n) = p$,

$ab + ac + ad$ &c. - - - - - - - $\left(n \times \frac{n-1}{2}\right) = q$,

$abc + abd$ &c. - - - - $n \times \frac{n-1}{2} \times \frac{n-2}{3} = r$,

($abcde$ &c. $n$), or the product of them all, being the coefficient of the last term. Now, as we have $(n)$ equations, and $(n)$ indeterminate quantities, it is evident, that by employing each equation successively to determine *one* quantity, the whole will be determined. But the equations are not *all* of the *same* degree: the first, is a simple equation: the second, being composed on one side wholly of products by two, is in degree a quadratic; the third, for the same reason, a cubic; and so on. If the first of these equations be used to determine $(a)$, we shall have ($a = p - b - c - d$ &c. $- n$); inserting that value for $(a)$ in the second equation, it becomes the quadratic ($pb - b^2 + pc - c^2 + pd - d^2 - bc - bd$ &c. $= q$). If that quadratic be solved to determine $(b)$, and the values of ($a$ and $b$) be inserted in the third equation, it becomes the cubic ($c^3$ &c. . . $= r$). Moreover, the quadratic having *two* roots, its solution will have introduced a quadratic surd. Before therefore we can proceed to employ the third equation to determine ($c$), it must be squared to clear it of that surd, and of course will then rise to the 6th degree. The solution of such a dimension (if admitted for the present to be equally possible) must introduce higher radicals; and, by the intrusion of these superfluous roots at every stage, our labour increases, instead of diminishing. This is the difficulty alluded to before; and, as we have appropriated already *all* our subordinate equations, we have nothing to oppose it. It therefore seems hopeless, to expect to make any general impression upon indeterminate equations, without more help, beyond the mere knowledge of the constitution of the coefficients.

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* The nature of the roots is not material in this place; whether affirmative or negative, real or imaginary, they have just the same operation in forming the coefficients of the equation. I have however *throughout* chosen, wherever I could, to give examples capable of being tried by real and affirmative roots; and, for that purpose, have uniformly made the signs of the coefficients alternately affirmative and negative.

11. This difficulty, however, is wholly removed by the least circumstance that discloses any particular relation amongst the coefficients of an equation, independent of the general law of their construction. This, of course, whenever it occurs, furnishes new conditions and means of comparing the terms. Every particularity in the coefficients that gives specific varieties to the forms of equations, must, from the nature of their construction, have its source in some particular relation between two or more of the roots, and therefore, as far as that relation extends, detects them infallibly. The observation of the forms and relations of the coefficients under different species of equations, and the correspondent inferences to be drawn, as to the connection of their roots, would form a curious and very useful part of a complete treatise upon the whole doctrine of equations, which is a work much wanted. The most striking of these relations will be obvious, or familiar, to the reader who has at all considered the nature of the subject; such as, that equations deficient in every alternate term arise from pairs of equal roots

with opposite signs ($\pm a, \pm b$ *&c.*); that those whose terms on both sides the middle term are alike (which are generally called recurring equations) arise from pairs of roots, of which each pair contains a quantity and its reciprocal $\left(a\ \frac{1}{a},\ b, \frac{1}{b}\ \text{\&c.}\right)$; together with MACLAURIN's demonstration of the particularities of the coefficients when an equation has equal roots.* And the extent to which these notices might easily be carried, from observation of the effects of the different sorts of proportion, and all other relations, is prodigious. But my present concern is merely with the result, supposing from any means a relation to be previously discovered affecting any number of the roots. For example,—suppose, in the above given equation, ($x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4}$ *&c.* $= 0$), whose roots we called ($a, b, c, d$ *&c.* . . . $n$), we happened to know that two of the number ($a$ and $b$) were equal; then, since they might *both* be expressed by the same character, the ($n$) roots of the equation might now be represented by only ($n - 1$) distinct characters; and therefore, of the subordinate equations derived from the construction of the coefficients, *two* might be employed to determine *one root.* ($a$ and $b$) being equal, the equation furnished by the value of the coefficient ($p$), and also that furnished by the coefficient ($q$), may be *both together* used to determine the same quantity. But, if any quantity ($a$) be a root of an equation, the simple equation ($x - a = 0$) must be a divisor of that equation;† therefore *here* ($x - a$) must be a common divisor of the two equations furnished by ($p$ and $q$).

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* Vide MACLAURIN's Algebra, chap. iv. p. 162, *et infra.*

† Vide SANDERSON's Algebra, Vol. ii. p. 679, 680, Art. 432, and all algebras on the method of divisors.

and consequently may be found, without *resolving* either of them, by continual division or subtraction, according to the ordinary rule for finding the common measure.*

12. Any other relation *from the knowledge of which one character may be made to represent two or more roots*, evidently answers the same end. Indeed all relations of that kind may be converted into equality itself, by taking, instead of the given equation, some other properly derived from it. Thus if, instead of ($a$ and $b$) being the same, ($b$) had been supposed the negative of ($a$) or ($-a$), and then, instead of the former equation, that of the squares of the roots were taken, the relation would be made equality; for ($a$) and ($-a$) have the same square. If arithmetical proportion was known to be the relation of any number of the roots, by taking the equation of their differences, it would also be converted into equality.

13. If three or more roots, or any number of parcels of roots, are known to be related, and their common relation be used to represent them, of course the number of distinct characters to be determined will proportionably be diminished: and, as the number of subordinate equations furnished by the coefficients remains always the same, while the dimension of the proposed equation is unaltered, more of them may be used together to discover the related roots, and their investigation be proportionably facilitated. *This single observation*, in the hands of a

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* Vide SANDERSON's Algebra, (quarto ed. Vol. i. p. 86, 87, 88,) where the rule is well given; and MACLAURIN's Algebra, (P. II. cap. iv. p. 162.); or Mr. HELLINS's Essay upon the Reduction of Equations having equal Roots. But of the last it should be observed, that some qualification must be made to the assertion, that the reduction may be carried on till a *simple* equation is obtained. In cases where there is only *one* pair of roots equal, that proposition is undoubtedly true; but,

skilful analyst, is sufficient for the reduction, if not the solution, of any particular *numeral* equation whatsoever, and the more so the larger its dimension: for, from the endless variety of relations numbers bear to each other, hardly any set of them can occur, as the coefficients of an equation, or perhaps exist, that, upon being compared, do not exhibit some peculiarity (of greater or less extent) sufficient to afford a clue to the correspondent relation in their roots. And, if no such clue is immediately given by the equation itself, taking the equation of the differences or sums in pairs, or of the squares, &c. of the roots, will soon find one. But, as peculiarities of that sort (though never so frequent) may be deemed always accidental, and evidently, no general method can be founded upon them, *even where the coefficients are given*, it may be asked, How any use can be made of them in cases of indeterminate equations?

14. To this I answer, that there are some properties of quantities that depend only on the index of the equation, without any regard to the value of its coefficients; or, in other words, there are some peculiar properties which merely depend upon the *number* of any set of quantities, abstracted from all consideration of their nature and values. For example, two quantities ($a$) and ($b$) have their differences the same quantity ($a - b$), only taken both affirmatively and negatively, ($a - b$) and ($b - a$); when squared, these differences become equal; ($a^2 - 2ab + b^2$) is the square of both: therefore, let the quantities themselves be chosen as they may, the equation of the squares of their

if 2, 3, or more pairs of roots are equal, the reduction can only be carried down to a quadratic, cubic, &c. for, *every* pair of equal roots being equally to be found by the method, of course the final or resulting equation must be of a dimension as great as their number.

differences *must* have *both equal roots*, and consequently be reducible by the reasoning in Art. 11, 12, and 13. Again, *three* quantities, however distinct in themselves, give a set of differences marked with a peculiar relation, any two of them being equal to the third; $(a, b, c,)$ being three quantities, $(\overline{a-b} + \overline{b-c} = \overline{a-c})$. Also, if the three quantities be so chosen originally as to have their sum equal nothing, one of them must necessarily equal in magnitude the sum of the remaining two; and therefore, whether taken simply or summed in pairs, their relative magnitudes must remain the same. Again, *four* quantities, of any sort whatever, may be pursued to a constant relation, though somewhat more remote, and grounded upon very different causes; *viz.* $(a, b, c, d,)$ being four quantities, from their combinations by pairs $(ab, ac, ad, bc, bd, cd,)$ six in number, added together two by two, thus,

$$(ab + cd)$$
$$(ac + bd)$$
$$(ad + bc)$$

*three* quantities are formed, sufficiently distinguished from the group of similar combinations to be found separately, as will be shewn hereafter. And also, if the four quantities are originally so taken as to have their sum equal to nothing, their sums in pairs, though six in number, will be reduced to three in effect; for, if $(a + b + c + d = 0)$, by transposition,

$$(a + b = -c - d)$$
$$(a + c = -b - d)$$
$$(a + d = -b - c)$$

*i. e.* three of the six must be merely the negatives of the other three; which relation, if they are squared, will become equality, so that the number of distinct squares will be only three. These properties, though without any order or connection, and confined merely to particular ranks or numbers of quantities, being general to *all possible* or *imaginable* quantities of *those classes*, afford methods general, as to those degrees, but without producing any result *really general to equations at large.*

15. Having shewn that an indeterminate general equation cannot be resolved by any of the methods whose principle is yet known, because they are all grounded upon the assumption of some particularity, either inherent in the roots, or universally communicable to them, which, so far from being general, is seldom found, and absolutely incompatible with many sorts of roots; that the difficulty is in all cases the same,—the intrusion of superfluous roots and higher radicals; that a relation of any kind (when known) obviates that difficulty, as far as it extends; and that *some* orders of quantities have generally a *constant* and *necessary* relation, more or less remote, I proceed to examine, more minutely, the application of these observations to the several degrees of equations to which they materially apply.

## CHAP. II.

### *Of the Resolution or Reduction of Equations of particular Degrees.*

16. In examining those degrees of equations which submit to be resolved, I shall observe the same order as I did before; *i. e.* first consider the power of obtaining a general formula, or complete resolution; and, if that is not attainable directly, inquire by what general means the roots can be separately inves-

tigated, and what new forms they have taken, or what different functions of them are used in the operation.

17. If we resume the general indeterminate equation $(x^n - px^{n-1} + qx^{n-2} - rx^{n-3}\ \&c. = 0)$, and assign the progressive values (2, 3, 4 &c.) to the index ($n$), in the first case it will become the quadratic $(x^2 - px + q = 0)$. Now, as this equation has two roots, in order to obtain a general formula for its resolution, the first step that suggests itself is, to inquire what is necessary to construct a general representation of two quantities in a simple equation. Two quantities are known to be generally expressed by means of their sum and difference; that half their sum added to half their difference gives the greater, and the same quantities subtracted, the lesser. The sum being always the coefficient of the second term of the equation, *is given in all cases*, and *here* the difference is readily found; for, the square of the difference of any two quantities differs from the square of their sum by a constant quantity, *viz.* four times their product or the coefficient of the third term. If ($a$) and ($b$) be called the roots of the equation $(x^2 - px + q = 0)$; then

$(p = a + b)$ and $(p^2 = a^2 + 2ab + b^2)$,

$(q = ab)$ and $(-4q = \quad -4ab \qquad)$,

$$\text{whence}\quad \overline{(a^2 - 2ab + b^2} = \overline{a - b}|^2 = \overline{p^2 - 4q}\left\{\begin{array}{l}\text{the square}\\ \text{of the dif-}\\ \text{ference.}\end{array}\right.$$

The difference itself is therefore $(\sqrt{p^2 - 4q})$. And now, being possessed of the parts required to construct a general representation of the two quantities, we can at once complete the formula of general resolution of equations of this degree, *viz.*

$$\left(x = \frac{p \pm \sqrt{p^2 - 4q}}{2}\right).$$

*This*, as I observed before in Art. 6, is however the *same* quadratic, only reverted; for, the quadratic surd it contains is frequently incapable of further reduction. Therefore, generally speaking, the *degree* of the equation is not altered; only the place of the index, which being first affixed to the unknown quantity, is now transferred to the known ones. But nevertheless, this resolution is, in *all* cases, *equally true* and *direct;* for, involving no other radical than belongs to the degree it relates to, it faithfully exhibits the nature of the roots, and is always rational or real, or not, according as they are so.

18. If, instead of seeking, *a priori*, the formula of resolution, we attempt to find the roots simply, we may instantly trace a constant connection between them, or at least between their differences; which (however the quantities are varied) are always related in the same manner, being $(a-b)$ and $(-a+b)$ the same quantity with different signs, and consequently their squares precisely the same. From which it appears, that the equation of those differences will always want the second term or be a pure quadratic; and *that* of their squares will be a perfect binomial square, having both roots equal; which roots may therefore, by the reasoning in Art. 11, be certainly found. But the inference is just the same as before: the equation is not lowered in degree; the equal relation is brought no nearer than between the squares of the differences; and, when *they* are found, the same quadratic surd must be used to arrive at the roots themselves. This formula of resolution $\left(x=\frac{p\pm\sqrt{p^2-4q}}{2}\right)$ is the same given for quadratics in every algebra; but it is not usually remarked, or perhaps understood, that the whole operation, however varied in appearance by setting about to complete the square (as it is called) or to destroy the second term,

is merely employed to obtain the difference of the roots; that (upon analysing the formula) the part under the vinculum is always that difference, and nothing else, and *why* it must be so.

19. Next, let ($n = 3$), and the equation be the complete cubic ($x^3 - px^2 + qx - r = 0$). If we make it our first step here, as in the last case, to inquire what is necessary to construct a general representation of three numbers in simple equations, we shall find it must consist of the same parts, the sum and the differences: but, as the differences increase in number, to show the order in which they are taken, and the law they observe progressively, I shall subjoin a general table of the simple representation of the different orders of quantities. As in every equation the sum of the roots is always given, I shall, for greater simplicity in my table, suppose it always to vanish. If then there be a series of general equations, beginning with a quadratic, and proceeding upwards with progressive indexes, in all of which the coefficient of the second term ($p$) be taken $= (0)$, and (A) be supposed a difference of the roots of the first, (A) and (B) two of the differences of the roots of the second, (A, B, C,) three differences of those of the third, and so on; in taking of which differences, no other caution is necessary than that they should be similarly situated, *viz.* all derived by comparing the same individual root with the remaining ones, as if ($a$) be taken as a root, and ($\overline{a - b}$) be the first difference, ($\overline{a - c}$, $\overline{a - d}$, $\overline{a - e}$ &c. . . . $\overline{a - n}$), having *all* the *same antecedent* letter (whose number will always be ($n - 1$)), must be the rest; *then*, the table will be as follows:

## *Table of the simple Representation of the Roots of Equations of progressive Indexes.*

In quadratics,

$$x = \pm \frac{A}{2}$$

In cubics,

$$x = \left\{ \begin{array}{l} \dfrac{A+B}{3} \\ \dfrac{-A-B}{2 \cdot 3} + \left\{ \begin{array}{l} + \dfrac{A-B}{2} \\ - \dfrac{A+B}{2} \end{array} \right. \end{array} \right.$$

In biquadratics,

$$x = \left\{ \begin{array}{l} \dfrac{A+B+C}{4} \\ - \dfrac{A-B-C}{3 \cdot 4} \left\{ \begin{array}{l} + \dfrac{A-B}{3} - \dfrac{B+C}{3} \\ + \dfrac{A-C}{3} + \dfrac{B-C}{3} \\ - \dfrac{A+B}{3} - \dfrac{A+C}{3} \end{array} \right. \end{array} \right.$$

In the fifth degree,

$$x = \left\{ \begin{array}{l} \dfrac{A+B+C+D}{5} \\ - \dfrac{A-B-C-D}{4 \cdot 5} \left\{ \begin{array}{l} + \dfrac{A-B}{4} - \dfrac{B+C}{4} - \dfrac{B+D}{4} \\ + \dfrac{A-C}{4} + \dfrac{B-C}{4} - \dfrac{C+D}{4} \\ + \dfrac{A-D}{4} + \dfrac{B-D}{4} + \dfrac{C-D}{4} \\ - \dfrac{A+B}{4} - \dfrac{A+C}{4} - \dfrac{A+D}{4} \end{array} \right. \end{array} \right.$$

In the ($n$th) degree, or generally,

$$x = \left\{\begin{array}{l} \dfrac{A+B+C+D+E\ \&c.\ \ldots\ldots\ \overline{(n-1)}}{n} \text{ in number} \\ \\ \dfrac{-A-B-C-D-E\ \&c.}{\overline{n-1}\times n} \left\{\begin{array}{l} +\dfrac{A-B}{n-1}-\dfrac{B+C}{n-1}\ \&c.\ \overline{n-2} \text{ in number} \\ +\dfrac{A-C}{n-1}+\dfrac{B-C}{n-1}\ \&c. \\ +\dfrac{A-D}{n-1}+\dfrac{B-D}{n-1}\ \&c. \\ +\dfrac{A-E}{n-1}+\dfrac{B-E}{n-1}\ \&c. \end{array}\right. \end{array}\right.$$

20. The inspection of the table shews us, that in *all* cases, to construct a *general simple representation* of any number of quantities, and consequently to construct a *direct resolution* of their equation, we must first find a certain number of their differences; but we have no *general means* of separating particular differences from the rest; and the whole number of differences increases in a proportion so much greater than the number of quantities, that the former difficulty recurs, the previous steps involve higher dimensions than the original equation. The original index being ($n$), that of the equation of the difference of the roots is ($n \times \overline{n-1}$). However, from the nature of differences, (being taken both affirmatively and negatively,) all equations formed from them must (as observed of quantities of that sort in Art. 11.) be universally *deficient* in *every alternate* term, which brings their equation to the *form* of equations of *only half* their own index, or $\left(n \times \frac{\overline{n-1}}{2}\right)$: but, in this case, their differences are six, and their equation, *with that consideration*, is reduced no lower than a cubic form, which is the same degree with the proposed equation; therefore, it does not appear

that we can be enabled, *a priori*, to determine the formula of any direct resolution of this case.

21. Let us then try to trace some relation which may convert some or all of the roots, or some regular function of them, into equal quantities; when, the equation of that function having equal roots, of course those roots will be separately deducible, as shewn in Art. 11, 12. In Art. 14, we may remember, two particularities were mentioned to belong to three quantities, *viz.* that their differences were so related as to be every two of them equal to the third; and that, if the quantities themselves have their sum equal to nothing, two of *them* also must equal the third, and their magnitude be respectively the same, whether they are taken simply, or summed in pairs. To avail ourselves of *both* these properties, let us suppose the second term to be expunged from the given equation, (which we know may always be effected,) its form will then be ($x^3 - qx + r = 0$),* and the sum of its roots equal to nothing. Let ($a$) and ($b$) be two of its roots, the third will therefore be ($-a-b$); take their sums by two ($-a,\ -b,\ a+b$); take their differences ($2a+b,\ a+2b,\ a-b$) *and their negatives*, which may be divided into two sets whose sum is nothing, like that of the roots, *viz.*

$$\left\{\begin{array}{ccc} a-b, & a+2b, & -2a-b \\ -a+b, & -a-2b, & 2a+b \end{array}\right\}.$$

So that, from the given equation we derive three others, which make a set of four exactly similar.

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* Besides expunging ($p$), the sign of ($q$) has been changed; because, in cases of real roots, it will invariably become negative upon destroying the second term. Vide note in p. 275.

1st. ($x^3 - qx + r = 0$) the given equation.
2d. ($x^3 - qx - r = 0$) that of its roots summed in pairs.

$$\left.\begin{array}{ll}\text{3d.} & x^3 - 3qx + \sqrt{4q^3 - 27r^2} = 0 \\ \text{4th.} & x^3 - 3qx - \sqrt{4q^3 - 27r^2} = 0\end{array}\right\}\text{two similar equations,}$$

formed by dividing ($x^6 - 6qx^4 + 9q^2x^2 - \overline{4q^3 + 27r^2} = 0$), the equation of the differences, into two wanting the second term.

22. Now, leaving these considerations for a moment, let us speculate upon the further reduction of the equation. If, instead of the present form ($x^3 - qx + r = 0$), ($q$) could be supposed to vanish as well as ($p$), a still more powerful additional relation would be given the roots; for, the equation being *then* a pure cubic, ($x^3 = \pm r$), its roots would obviously be the cube roots of ($r$), and *all cube roots* are *alike.* If ($r$) be a cube, and ($\sqrt[3]{r}$) be one of its roots, the remaining two are $\left(\frac{-1+\sqrt{-3}}{2} \times \sqrt[3]{r} \text{ and } \frac{-1-\sqrt{-3}}{2} \times \sqrt[3]{r}\right)$, let ($r$) be any quantity whatsoever, real or imaginary. But it is clear, from what has been before observed in Art. 9, that this reduction is not generally possible, since it supposes two contiguous intermediate terms to vanish together, which real roots do not admit of: it must therefore be effected by means of some imaginary assumption. Those who are conversant in the use of impossible quantities, will at once perceive, that the addition or subtraction (which in surd quantities is always the same thing, as they are equivocal in sign,) of the imaginary surd $\left(\sqrt{-\frac{q}{3}}\right)$ to each root of the equation, will infallibly cause ($q$) to vanish, but the new roots $\left\{\begin{array}{l} a + \sqrt{-\frac{1}{3}q}, \\ b + \sqrt{-\frac{1}{3}q}, \end{array}\right. -a - b + \sqrt{-\frac{1}{3}q})$ so formed, would not have their sum equal to nothing; and

therefore, in destroying the third term, the second would be revived, so that nothing would be gained.

23. To understand how this difficulty is ever removed, let us examine particularly some equation that wants both second and third terms, and observe accurately the constitution of its roots. The simplest of the kind is the pure cubic ($x^3 = 1$), whose roots are $\left(1, \frac{-1 \pm \sqrt{-3}}{2}\right)$; but, to avoid fractions in the roots, let us take ($x^3 = 8$), whose roots are ($2, -1 \pm \sqrt{-3}$). Distinguishing the real and the imaginary parts, the real are ($2, -1, -1$); the imaginary are ($\pm \sqrt{-3}$ or $\pm 3 \times \sqrt{-\frac{1}{3}}$), which are the *differences of the real parts,* multiplied by the imaginary surd ($\sqrt{-\frac{1}{3}}$). It appears, therefore, that the roots of a pure cubic are compounded of the roots of some affected cubic, added to their differences drawn into the imaginary surd ($\sqrt{-\frac{1}{3}}$). The real parts ($2, -1, -1$) are the roots of the cubic equation ($x^3 - 3x + 2 = 0$). The imaginary, of the equation ($x^3 + 3x + * = 0$), or the roots of the *similar* equation of the differences of the former, *viz.* ($x^3 - 9x + * = 0$), drawn into the ($\sqrt{-\frac{1}{3}}$); and, from *their* addition are formed the roots of the pure cubic ($x^3 = 8$). In constructing which, it is material to observe, *each root* of the first equation is joined to the difference of the remaining pair; but it may be remembered, that *three* quantities whose sum is nothing, are the same when summed in pairs, *i. e.* each is (in quantity) the sum of the other two, therefore, each difference is in fact added to the *sum of the same quantities;* and, if the question were proposed to reduce the equation ($x^5 - 3x + 2 = 0$) to a pure cubic, the rule furnished by this example would be, to find the equation of the sum of its roots in pairs, which, by the last Article, is

$(x^3 - 3x - 2 = 0)$; to find the *similar* equation of their differences $(x^3 - 9x + * = 0)$; and, to find the equation produced by the quantities formed from the addition of the roots of the one to those of the other multiplied into the imaginary surd $(\sqrt{-\tfrac{1}{3}})$. The equation last found would, however, be of the dimension of the 9th power, at least: for, the addition of each root of the second equation to every separate root of the first, produces a separate quantity: thus,

$$\begin{array}{l} (2, -1, \quad - \quad 1) \\ \textit{being the roots of the 1st.} \\ (0, +\sqrt{-3}, -\sqrt{-3}) \\ \textit{those of the 2d.} \quad - \end{array} \left\{ \begin{array}{lll} \overline{2+0} & , -\overline{1+0} & , -\overline{1+0} \\ 2+3\sqrt{-\tfrac{1}{3}} & , -1+3\sqrt{-\tfrac{1}{3}} & , -1+3\sqrt{-\tfrac{1}{3}} \\ 2-3\sqrt{-\tfrac{1}{3}} & , -1-3\sqrt{-\tfrac{1}{3}} & , -1-3\sqrt{-\tfrac{1}{3}} \end{array} \right\}$$

will be the nine quantities formed by their addition. But we have a decisive clue to distinguish *some* from the rest; for we know, that if we find the equation of the cubes of those quantities, *it must have three equal roots*; for, every time the *sum* of two of the roots of the first equation meets its *own* difference, it will constitute a cube root of (8), and therefore, the equation $(x^3 - 8 = 0)$ will be three times contained in the resulting equation of cubes. That equal root being discovered by the method of finding equal roots, so often alluded to before, reduces the equation $(x^3 - 3x + 2 = 0)$ to the pure cubic $(x^3 = 8)$.

24. The instance in the last article, of the reduction of the equation $(x^3 - 3x + 2 = 0)$ to a pure cubic, by means of the equation $(x^3 + 3x = 0)$, evidently depends upon the coefficient of the second term vanishing; and also, that of the third term being the same in both, but of opposite signs. For, the roots of the one, in their combinations by two, producing $(-3)$, and those of the other $(+3)$, of course destroy each

other; and, as the sums of both equal nothing, when added together their sum will still be nothing; so that *no new second term can arise*, as in Art. 22. If we now return to the considerations in Art. 21, where we shewed how to derive from every cubic equation ($x^3 - qx + r = 0$) wanting the 2d term, a similar equation ($x^3 - 3qx + \sqrt{4q^3 - 27r^2} = 0$), being the equation of three of the differences of the roots of the former, so arranged as to want the second term also, we may perceive that, to render the third term the same in both, we need only divide the roots of the latter by ($\sqrt{3}$), or, which is the same thing, multiply them into the ($\sqrt{\frac{1}{3}}$). For, the equation ($x^3 - 3qx + \sqrt{4q^3 - 27r^2} = 0$), when its roots are multiplied by the ($\sqrt{\frac{1}{3}}$), becomes $\left(x^3 - qx + \frac{\sqrt{4q^3 - 27r^2}}{3\sqrt{3}} = 0\right)$.* If, by the same reason, they had been multiplied by the ($\sqrt{-\frac{1}{3}}$), it would be $\left(x^3 + qx + \frac{\sqrt{4q^3 - 27r^2}}{3\sqrt{-3}} = 0\right)$; where the sign of the coefficient of ($x$) is opposite to that of ($q$) in the given equation. Therefore, the roots of the equation ($x^3 - qx + r = 0$), and that of its differences, multiplied into the imaginary surd $\sqrt{-\frac{1}{3}}$, *viz.* $\left(x^3 + qx + \frac{\sqrt{4q^3 - 27r^2}}{3\sqrt{-3}} = 0\right)$, will, by being added together, (according to the method in the last Article,) lead to a reduction of that equation to a pure cubic; *i. e.* the equation formed from their addition will have three roots, whose cubes are the same.

25. The analysis of the pure cubic gives us the following *general properties*, belonging to any set of those equations whose sum is nothing.

*Viz.* 1st. If three such quantities ($a, b, -a-b$) be added in pairs, and three of their differences be also taken so as to have their sum nothing, ($a-b$, $a+2b$, $-2a-b$); if *then* each sum be formed into a binomial, by joining to it its correspondent difference multiplied by the imaginary surd $\sqrt{-\frac{1}{3}}$, the quantities so formed

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* Vide SANDERSON's Algebra, Vol. II. p. 688; and HALE's Analysis, p. 146.

$$a+b+\overline{a-b}\sqrt{-\tfrac{1}{3}}$$
$$-a+\overline{a+2b}\sqrt{-\tfrac{1}{3}}$$
$$-b-\overline{2a-b}\sqrt{-\tfrac{1}{3}}$$ will have the *same cube.*

Example 1st,

$$\overline{a+b}+\overline{a-b}\sqrt{-\tfrac{1}{3}}$$
$$\overline{a+b}+\overline{a-b}\sqrt{-\tfrac{1}{3}}$$
$$\overline{a+b}^{2}+2\overline{a+b}\times\overline{a-b}\sqrt{-\tfrac{1}{3}}$$
$$-\frac{\overline{a-b}^{2}}{3}$$
$$\frac{2a^{3}+8ab+2b^{2}}{3}+2.\overline{a+b}\times\overline{a-b}\sqrt{-\tfrac{1}{3}}=\ldots\overline{a+b+\overline{a-b}\sqrt{-\tfrac{1}{3}}}^{2}$$
$$\overline{a+b}+\overline{a-b}\sqrt{-\tfrac{1}{3}}$$
$$\frac{2a^{3}+10a^{2}b+10ab^{2}+2b^{3}}{3}+2.\overline{a+b}^{2}\times\overline{a-b}\sqrt{-\tfrac{1}{3}}$$
$$+\frac{2.\overline{a+b}^{2}\times\overline{a-b}}{3}\sqrt{-\tfrac{1}{3}}$$
$$-\frac{2a^{3}+2a^{2}b+2ab^{2}-2b^{3}}{3}\qquad+\frac{4ab}{3}\times\overline{a-b}\sqrt{-\tfrac{1}{3}}$$
$$☞\ {*}\,\frac{12a^{2}b+12ab^{2}}{3}\,{*}+\frac{\overline{8a^{3}+12a^{2}b-12ab^{2}-8b^{3}}}{3\sqrt{-3}}=\overline{\overline{a+b}+\overline{a-b}\sqrt{-\tfrac{1}{3}}}^{3}$$
$$-a+\overline{a+2b}\sqrt{-\tfrac{1}{3}}$$
$$-a+\overline{a+2b}\sqrt{-\tfrac{1}{3}}$$
$$a^{2}-\overline{2a^{2}-4ab}\sqrt{-\tfrac{1}{3}}$$
$$\frac{-a^{2}-4ab-4b^{2}}{3}$$
$$\frac{2a^{2}-4ab-4b^{2}}{3}-\overline{2a^{2}-4ab}\sqrt{-\tfrac{1}{3}}=\ldots\overline{-a+\overline{a+2b}\sqrt{-\tfrac{1}{3}}}^{2}$$

$$\begin{array}{l}
\frac{2a^2-4ab-4b^2}{3}-\overline{2a^2-4ab}|\surd-\frac{1}{3}\\
-a+\overline{a+2b}|\surd-\frac{1}{3}\\
\hline
-\frac{2a^3+4a^2b+4ab^2}{3}+\overline{2a^3+4a^2b}|\surd-\frac{1}{3}\\
\quad\frac{2a^3+8a^2b+8ab^2}{3}-\frac{\overline{a+2b}|^3}{3}\ \surd-\frac{1}{3}\\
\qquad\qquad\qquad+a^2\times\overline{a+2b}|\ \surd-\frac{1}{3}\\
\hline
\end{array}$$

☞ $* \frac{12a^2b+12ab^2}{3}+\overline{\frac{8a^3+12a^2b-12ab^2-8b^3}{3\surd-3}}| = \ldots\overline{-a+\overline{a+2b}|\surd-\frac{1}{3}}|^3$

$$\begin{array}{l}
\quad-b-\overline{2a-b}|\surd-\frac{1}{3}\\
\quad-b-\overline{2a-b}|\surd-\frac{1}{3}\\
\hline
b^2+\overline{4ab+2b^2}|\surd-\frac{1}{3}\\
-\frac{b^2-4ab-4a^2}{3}\\
\hline
\frac{2b^2-4ab-4a^2}{3}+\overline{4ab+2b^2}|\surd-\frac{1}{3}=\ldots\ldots\overline{-b-\overline{2a-b}|\surd-\frac{1}{3}}|^2\\
-b-\overline{2a-b}|\surd-\frac{1}{3}\\
\hline
-\frac{2b^3+4ab^2+4a^2b}{3}-\overline{4ab^2-2b^3}|\surd-\frac{1}{3}\\
\quad\frac{2b^3+8ab^2+8a^2b}{3}+\frac{\overline{2a+b}|^3}{3}\surd-\frac{1}{3}\\
\qquad\qquad\qquad+b^2\times-\overline{2a-b}|\surd-\frac{1}{3}\\
\hline
\end{array}$$

☞ $* \frac{12a^2b+12ab^2}{3}+\overline{\frac{8a^3+12a^2b-12ab^2-8b^3}{3\surd-3}}| = \ldots\overline{-b-\overline{2a-b}|\surd-\frac{1}{3}}|^3$

The quantity

$$\frac{12a^2b+12ab^2}{3}+\overline{\frac{8a^3+12a^2b-12ab^2-8b^3}{3\surd-3}}|=4\times\overline{\overline{a^2b+ab^2}|+\overline{\frac{2a^3+3a^2b-3ab^2-2b^3}{3\surd-3}}}|$$

is therefore the common cube of those 3 binomials. *Q. E. M.*

Now let the equation of the 3 quantities $(a, b, -a-b,)$ be $(x^3-qx+r=0)$; then, by the construction of equations,

$(q=a^2+ab+b^2)$ and $(4q^3=4a^6+12a^5b+24a^4b^2+28a^3b^3+24a^2b^4+12ab^5+4b^6)$, also $(-r=a^2b+ab^2)$ and $\underline{(27r^2= \text{- - - - - - - -}\ 27a^4b^2+54a^3b^3+27a^2b^4\qquad)}$, whence $(4q^3-27r^2=4a^6+12a^5b-3a^4b^2-26a^3b^3-3a^2b^4+12ab^5+4b^6)$ and its square root, or $(\sqrt{4q^3-27r^2}=\overline{2a^3+3a^2b-3ab^2-2b^3})$. Therefore, the quantity

$$4\times\left|\overline{\overline{a^2b+ab^2}\,+\frac{\overline{2a^3+3a^2b-3ab^2-2b^3}}{3\sqrt{-3}}}\right|=4\times\left|\overline{-r+\frac{\sqrt{4q^3-27r^2}}{3\sqrt{-3}}}\right|.$$

The equation $(x^3-qx+r=0)$ may hence be reduced to a pure cubic of this form $\left(x^3=4\times\left|\overline{-r+\frac{\sqrt{4q^3-27r^2}}{3\sqrt{-3}}}\right|\right)$, which, when cleared of its irrational quadratic surd, becomes $\left(x^6+8rx^3+16r^2=\frac{-64q^3+432r^2}{27}\right)$, or $\left(x^6+8rx^3+\frac{64q^3}{27}=0\right)$; or, dividing its roots by (2) to reduce it still lower, $\left(x^6+rx^3+\frac{q^3}{27}=0\right)$, the common reducing equation obtained by CARDAN's rule.

*Example 2d.* Let $(1, 2, -3)$, the roots of the equation $(x^3-7x+6=0)$, be taken; the binomials formed from them will be, according to the directions of the rule,

$$\left.\begin{array}{l}1+2=+3\\1-2=-1\end{array}\right\}\ \underline{3-1\sqrt{-\tfrac{1}{3}}}$$

$$\left.\begin{array}{l}1-3=-2\\1+3=+4\end{array}\right\}\ \underline{-2-4\sqrt{-\tfrac{1}{3}}}$$

N. B. It is necessary to change the sign of the middle difference, because their sum must always $=0$).

$$\left.\begin{array}{l}2-3=-1\\2+3=+5\end{array}\right\}\ \underline{-1+5\sqrt{-\tfrac{1}{3}}}$$

$$\left\{\begin{array}{ccc}
3-1\sqrt{-\frac{1}{3}} & -2-4\sqrt{-\frac{1}{3}} & -1+5\sqrt{-\frac{1}{3}}\\
3-1\sqrt{-\frac{1}{3}} & -2-4\sqrt{-\frac{1}{3}} & -1+5\sqrt{-\frac{1}{3}}\\
\hline
9-6\sqrt{-\frac{1}{3}} & 4+16\sqrt{-\frac{1}{3}} & 1-10\sqrt{-\frac{1}{3}}\\
-\frac{1}{3} & -\frac{16}{3} & -\frac{25}{3}\\
\hline
\frac{26}{3}-6\sqrt{-\frac{1}{3}} & -\frac{4}{3}+16\sqrt{-\frac{1}{3}} & -\frac{22}{3}-10\sqrt{-\frac{1}{3}}\\
3-1\sqrt{-\frac{1}{3}} & -2-4\sqrt{-\frac{1}{3}} & -1+5\sqrt{-\frac{1}{3}}\\
\hline
26-18\sqrt{-\frac{1}{3}} & \frac{8}{3}-32\sqrt{-\frac{1}{3}} & \frac{22}{3}+10\sqrt{-\frac{1}{3}}\\
-2-\frac{26}{3}\sqrt{-\frac{1}{3}} & \frac{64}{3}+\frac{16}{3}\sqrt{-\frac{1}{3}} & \frac{50}{3}-\frac{110}{3}\sqrt{-\frac{1}{3}}\\
\hline
24-\frac{80}{3}\sqrt{-\frac{1}{3}} & 24-\frac{80}{3}\sqrt{-\frac{1}{3}} & 24-\frac{80}{3}\sqrt{-\frac{1}{3}}
\end{array}\right.$$

The quantity $\left(24-\frac{80}{3\sqrt{-3}}\right)$ is therefore the common cube of these 3 binomials.

*Example 3d* Let $(-1, -4, +5)$, the roots of the equation $(x^3 - 21x - 20 = 0)$, which are the differences used in the last example, be next taken;

$$\begin{array}{l} \left.\begin{array}{l} -1-4=-5 \\ -1+4=+3 \end{array}\right\} -5+3\sqrt{-\tfrac{1}{3}} = -5+1\sqrt{-3} \\[3em] \left.\begin{array}{l} -1+5=+4 \\ -1-5=-6 \end{array}\right\} +4+6\sqrt{-\tfrac{1}{3}} = \quad 4+2\sqrt{-3} \\[3em] \left.\begin{array}{l} -4+5=+1 \\ -4-5=-9 \end{array}\right\} +1-9\sqrt{-\tfrac{1}{3}} = \quad 1-3\sqrt{-3} \end{array} \left\{\begin{array}{rrr} -\ 5+1\sqrt{-3} & 4+2\sqrt{-3} & 1-3\sqrt{-3} \\ -\ 5+1\sqrt{-3} & 4+2\sqrt{-3} & 1-3\sqrt{-3} \\ \hline 25-10\sqrt{-3} & 16+16\sqrt{-3} & 1-6\sqrt{-3} \\ -\ 3 & -12 & -27 \\ \hline 22-10\sqrt{-3} & 4+16\sqrt{-3} & -26-6\sqrt{-3} \\ -\ 5+1\sqrt{-3} & 4+2\sqrt{-3} & 1-3\sqrt{-3} \\ \hline -110+50\sqrt{-3} & 16+64\sqrt{-3} & -26-6\sqrt{-3} \\ 30+22\sqrt{-3} & -96+8\sqrt{-3} & -54+78\sqrt{-3} \\ \hline -\ 80+72\sqrt{-3} & -80+72\sqrt{-3} & -80+72\sqrt{-3} \end{array}\right.$$

which last cube, if divided by (3), becomes $(-\frac{80}{3} + 24\sqrt{-3})$, or exactly the reverse of the first; the reason of which will be shewn in the next section of this Article.

The cube $(24 - \frac{80}{3}\sqrt{-\frac{1}{3}})$, when its equation $(x^3 = 24 - \frac{80}{3}\sqrt{-\frac{1}{3}})$ is made rational, gives the quadratic-formed equation of the 6th degree $(x^6 - 48x^3 - + 576 = -\frac{6400}{27})$; or, transposing all the terms to one side, and dividing it by (2), to reduce it, as before, $(x^6 - 6x^3 + \frac{343}{27} = 0)$; the same equation that results from the common methods.

2dly. The differences of the three differences $(a - b,\ a + 2b,\ -2a - b)$ are $(3a,\ 3b,\ 3 \times \overline{a + b})$, or merely three times the original quantities. Had, therefore, the differences themselves been taken as original quantities, and binomials been formed from *them*, according to the directions before observed, *those binomials*, and the ultimately resulting cubes, would differ from the former, in nothing essential but the place of the surd. The differences which were affected with it before, would *now* be clear; and the quantities themselves, or, which is the same thing, their sums in pairs, be affected with it. However, as these latter are to be multiplied by three, *that multiplication* will destroy the fraction when they come again to be multiplied by the surd $(\sqrt{-\frac{1}{3}})$, since $(3 \times \sqrt{-\frac{1}{3}} = \sqrt{-3})$. Wherefore, the same end, as to reducing the equation, will be obtained, *whether, after adding the sums* of the roots in pairs to their *respective differences*, we *multiply the sums by* $(\sqrt{-3})$, or *divide* the *differences* by it; as has been already shewn in the 3d Example to the last Section of this Article.

3dly. If any cubic equation wanting the second term, be transformed into the equation of that function of its roots, formed of the cubes of the binomials arising from joining the sum of each pair of roots to its correspondent difference drawn into the imaginary fractional surd $(\sqrt{-\frac{1}{3}})$, or each difference to its correspondent sum drawn into the surd $(\sqrt{-3})$, the transformed equation will have among its roots three equal cubes; by finding which, according to the methods of finding equal roots, the equation is reduced to a pure cubic.

4thly. The roots of a cubic equation may be *all* real; or only one of them real, and the remaining *two* imaginary. If only one be real, they will be of this form $\left(a, -\frac{a \pm b\sqrt{-1}}{2}\right)$; and, by taking their sums and differences according to the rule, and multiplying the latter into the $(\sqrt{-\frac{1}{3}})$, *one* of the resulting binomials will be real, and the other two imaginary: the cube produced by them will therefore be real. When all the roots are real, *if two be equal, one* difference necessarily vanishes; wherefore, the imaginary factor will only appear about the two that remain; and here again, the cube produced will be real. But, if all the roots are real, and unequal, their sums and differences will *all* be real: whence, *all* the binomials will involve the imaginary surd; which constitutes the irreducible case.

To give examples of this, let, 1st. $(x^3 - 2x + 4 = 0)$, a cubic equation, whose roots are $(2, -1 + \sqrt{-1}, -1 - \sqrt{-1})$; the binomials constructed by taking their sums and differences as before, will be

$$\left.\begin{aligned} 2 - \overline{1 + \sqrt{-1}} &= \overline{1 + \sqrt{-1}} \\ 2 + \overline{1 - \sqrt{-1}} &= \overline{3 - \sqrt{-1}} \end{aligned}\right\}$$

$$\left.\begin{aligned} 2 - \overline{1 - \sqrt{-1}} &= \overline{1 - \sqrt{-1}} \\ 2 + \overline{1 + \sqrt{-1}} &= \overline{3 + \sqrt{-1}} \end{aligned}\right\}$$

$$\left.\begin{aligned} -\overline{1 + \sqrt{-1}} - \overline{1 - \sqrt{-1}} &= \quad -2 \\ -1 + \sqrt{-1} + 1 + \sqrt{-1} &= \quad 2\sqrt{-1} \end{aligned}\right\}$$, which last binomial $\overline{-2 + 2\sqrt{-1}}$, when the latter quantity $(2\sqrt{-1})$ is drawn into the imaginary surd $(\sqrt{-\tfrac{1}{3}})$, becomes $\left(-2 - \frac{2}{\sqrt{3}}\right)$, a real quantity.

2dly. Let $(x^3 - 3x + 2 = 0)$ be proposed, whose roots have been, in Art. 23, shewn to be $(2, -1, -1)$. Here

$$\left.\begin{aligned} 2 - 1 &= +1 \\ 2 + 1 &= +3 \end{aligned}\right\}$$

$$\left.\begin{aligned} 2 - 1 &= +1 \\ 2 + 1 &= +3 \end{aligned}\right\}$$

$$\left.\begin{aligned} -1 - 1 &= -2 \\ -1 + 1 &= \quad 0 \end{aligned}\right\}$$. This latter binomial must evidently remain real, since the difference into which the imaginary factor was to have been drawn vanishes.

3dly. Let $(x^3 - 7x + 6)$ be given, whose roots are $(1, 2 - 3)$. The binomials derived from these have been before given, in the 2d Example to the first Section of this Article; and the cube they produce shewn to be $\overline{(24 - \frac{80}{3}\sqrt{-\frac{1}{3}})}$, the cube root of which cannot be extracted; it being from the quadratic surd, it involves, in truth, *not a cube*, but a *truncate sixth* power in a cubic shape: and when, to remove its equivocal state, it is made rational, shews itself to be properly the sixth power equation $(x^6 - 6x^3 + \frac{343}{27} = 0)$, as before demonstrated.

26. This is the common reduction of a cubic equation, to one of the sixth degree but in form a quadratic, obtained, by clearing of its quadratic surd, the pure cubic formed by either of the two sets of binomials before described; and this is the only reduction of it yet discovered. Perhaps the method called CARDAN's rule, is the shortest mode of effecting this reduction; but I am not aware, that the *real principle* upon which it is founded has been any where fully analysed and explained, except in the foregoing investigation of it. The ordinary expositions of it certainly disclose nothing of the principle, and are even in many respects faulty; for they treat it as the effect of a supposition or lucky conjecture, when, in fact, there is no supposition or conjecture made; a regular clue, furnished by certain demonstrable peculiarities in some functions of this order of quantities, being pursued, till such a relationship amongst the roots may be inferred, as may be converted into equality at some known period. They also fail to account for the most striking part of the result; the irreducibility happening uniformly in cases where it has been supposed least to be expected, *i. e.* when the roots are real; which they refer to a particular limitation

in one of the steps taken, when it is, in truth, of much deeper origin than any particular method, being the necessary consequence of the constitution of the cube power.

27. The result of these observations upon cubic equations shews, that *directly* they are not resolvable, *i. e.* they cannot, like quadratics, be always brought to a mere extraction of their correspondent root: that, however, by means of the peculiarities inseparable from the number of *three quantities*, a relation is discoverable, which inevitably gives equal roots to the equation of the cubes of a particular function of them; but that, that function involves sometimes a quadratic surd which was not in the roots themselves, but arose from the form necessary to be given them; that the equal relation not taking place in any case, till the cube of that function, and, in some cases, not being *rational*, till the square of that cube, the equation is not lowered in degree, by the operation, but rather increased.

28. Let ($n = 4$), and the equation become the general biquadratic ($x^4 - px^3 + qx^2 - rx + s = 0$), the number of differences are twelve; we cannot, therefore, hope to obtain a direct simple resolution. But, in Art. 14, two peculiarities belonging to sets of four quantities were pointed out, from which it is easy to obtain a reduction of the equation to a cubic form. The *first* peculiarity there mentioned, was shewn to subsist among the sums of the combinations of the roots in pairs. If ($a, b, c, d,$) be supposed the roots of the given equation, and their combinations by two ($ab, ac, ad, bc, bd, cd,$) be summed in pairs, though the number of quantities so formed are no fewer than 30, yet there is an evident distinction observable amongst them; for, in some, (the first six,) *no* letter occurs twice. If, therefore, instead of simply requiring the

sums of the combinations of the roots in pairs, that function of the roots had been required, consisting of the sums of these combinations, *into the forming of which no root enters twice,* only six out of the whole number of combinations of the kind would answer that condition; and those six would be the same *three* repeated, for ($ab + cd$, and $cd + ab$ *&c.*) are the same quantities. So that the three quantities ($ab + cd$, $ac + bd$, $ad + bc$,) would be the functions required, and all of the kind that can be made. Now, there is *no proposition* in the theory of equations more certain, than that the equation of any regular function of the roots may always be found by means of the known values of the coefficients.* As there are but three functions in this case, the resulting equation must consequently be a cubic; and, by taking the several combinations of the quantities ($ab + cd$, $ac + bd$, $ad + bc$,), we may obtain their equation,

$$\text{viz. } \underline{(x^3 - qx^2 + \overline{pr - 4s}\rvert x - p^2 s + 4qs - r^2 = 0)}.$$

Therefore, *the finding the equation of that function of the roots of a biquadratic which arises from its combinations by two summed in pairs, so however that no root shall occur twice in any such sum, reduces the biquadratic to a cubic.*

29. Another peculiarity of four quantities is also given in Art. 14, *i. e.* that if taken originally so as to have their sum equal to nothing, the six quantities formed from their sums in pairs, will be *the same three* quantities taken both affirmatively and negatively. Then we know, by the reasoning in Art. 11, *the equation of those quantities (though of the sixth degree) will want every alternate term, or be of a cubic form;* accordingly, the equation of the function of the roots formed by sum-

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* WARING'S *Med. Algeb.* cap. i. p. 1. *et infra.*

ming them in pairs, is $\overline{x-\frac{p}{2}}\Big|^{6} - \Big|\overline{2q+\frac{3}{4}p^{2}}\Big| \times \overline{x-\frac{p}{2}}\Big|^{4} + \overline{q^{2}-4s-qp^{2}+pr+\frac{3}{16}p^{4}}\Big| \times \overline{x-\frac{p}{2}}\Big|^{2} - \Big|\overline{\frac{p^{3}}{8}-\frac{p^{4}}{2}+r}\Big|^{2} = 0$,* which, when ($p$) is supposed to vanish, becomes ($x^{6} - 2qx^{4} + \overline{q^{2}-4s}\,\big|x^{2} - r^{2} = 0$).

30. These two methods, *one* applying to the biquadratic equations complete in their terms, and *the other* to those from which the second term has been expunged, are all that have yet been discovered; and, notwithstanding the number of different methods attributed to different writers, which from their manner of setting out appear distinct, they will all be found to resolve themselves, *in principle*, into one of these. Dr. HUTTON's Mathematical Dictionary, under the article Biquadratic Equations, gives four methods; *viz.* FERRARI's, DES CARTES's, EULER's, and SIMPSON's; to which may be added another by Dr. WARING,† and perhaps many more. They proceed upon a variety of different contrivances; but, when analysed, and the real object gained is viewed apart from the process that led to it, FERRARI's, which is the oldest, *and does not require the extinction of the second term*, will be seen to produce the cubic ($x^{3} - qx^{2} + \overline{pr-4s}\,\big|x - p^{2}s + 4qs - r^{2} = 0$); and DES CARTES's, *which supposes the second term to be first destroyed*, terminates in the cubic-formed equation of the sixth degree ($x^{6} - 2qx^{4} + \overline{q^{2}-4s}\,\big|x^{2} - r^{2} = 0$). The rest produce cubics, or cubic-formed sixth powers, whose roots are some parts or multiples of this last; except WARING's method, *which does not expunge the second term*, and *therefore* produces a cubic whose roots are parts of the first. But, whether the resulting equation be

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* WARING's *Medit. Algeb.* p. 133.

† Ibid. p. 138; and the Appendix to Dr. HUTTON's Dictionary.

that of the function, formed by summing the combinations by two of the roots in pairs, or summing the roots *themselves* in pairs, or the equation of the halves, or quarters, or doubles, trebles, *&c.* of those functions, is immaterial; *no new function* is employed, *no other principle* put in action, than what is derived from the general properties of this degree of quantities here explained.

31. Biquadratics being generally thus reducible to cubics, of course, by resolving those cubics, distinguishing what function their roots are of the roots of the original biquadratic, they may *all* be found; and, for practical utility, there is no preference to be made of either of the two methods; for, the first, though a *real cubic*, being formed from *products* of the roots, it requires a quadratic equation to obtain them after the cubic is resolved; whereas the *second*, though an equation of the sixth power, being formed from simple addition of the roots, gives them at once. But, as both these cubics necessarily have all their roots real, when those of the given biquadratic are so, and the resolution of cubics is in that case imaginary, it follows, that *no biquadratic having all its roots real*, can admit of a real solution by *either* of these methods.

32. The formula expressing the actual resolution of a biquadratic has not been given; the writers upon algebra going no further than to point out the cubics by means of which such a resolution may be obtained. To be sure, such a formula would be very long, and (till the imperfection in the cubic resolution, which must make a large part of it, can be removed,) embarrassed with radicals, so as to be of little practical use; but it would be a valuable accession to the theoretical part of algebra, to have the analysis of this degree carried as far as that of the

preceding, by developing every part of the functions that enter into the resolution, so as to be able to compose it at once, or make a complete reduction of the equation, without the intervention of any other steps.

33. Let $(n)$ be taken $= (5)$, or any higher number. *Here*, the number of differences is increased to twenty; and, the higher we go the more they increase, so that a *direct simple resolution* is out of the question. Nor are we yet acquainted with any peculiarity attending five, or any higher number of quantities, upon which we can ground a relation to effect a reduction of any sort; wherefore, no method is known for equations of this and the higher orders. Whether any may *ever* be discovered, it is not easy to pronounce: if the reasoning from Art. 8 to Art. 15, of this Paper, be correct, there can be no chance, until some peculiar property of quantities of this class can be hit upon. It is perhaps a discouraging presumption against the existence of any such property, that no art nor labour has hitherto afforded the least clue to lead to one; but, on the other hand, it is impossible to tell what general properties of quantity may remain to be discovered; and, from the great distance the peculiarities of the degrees we have treated of lie from the surface, and their total want of order or connection with each other, it may be justly expected those of the higher degrees may lie still more detached and remote, beyond any efforts that have yet been made upon the subject. The proper method to proceed seems therefore to be, *abandoning all projects for the general resolution of equations, to investigate regularly the abstract properties of each separate order or number of quantities, turning them into all shapes, sifting all their combinations, and constructing and examining the equations of different complex functions of them, in order to see*

*if latent peculiarities be not to be traced out in some of them.* Wherever any distinguishing property is found, it will, by the principles here explained, infallibly lead to some method for the degree to which it belongs; and, whoever may be fortunate enough to discover any such property, in five, six, or any higher order of quantities, will have the honour of removing the important and hitherto impenetrable barrier, which has so long obstructed the farther improvement of algebra.

# XVII. *On different Sorts of Lime used in Agriculture. By* Smithson Tennant, *Esq. F. R. S.*

Read June 6, 1799.

I was informed last summer, that in the neighbourhood of Doncaster, two kinds of lime were employed in agriculture, which were supposed to differ materially in their effects. One of these, which was procured near the town, it was necessary to use sparingly, and to spread very evenly over the land; for it was said that a large proportion of it, instead of increasing, diminished the fertility of the soil; and, that wherever a heap of it was left in one spot, all vegetation was prevented for many years. Fifty or sixty bushels upon an acre, were considered to be as much as could be used with advantage. The other sort of lime, which was obtained from a village near Ferry-bridge, though considerably dearer, from the distant carriage, was more frequently employed, on account of its superior utility. A large quantity was never found to be injurious; and the spots which were entirely covered with it, instead of being rendered barren, became remarkably fertile. The different properties ascribed to these two kinds of lime were so very distinct, that it seemed probable they could not be imaginary; and it therefore appeared to be worth the trouble of ascertaining them more fully, and of attempting to discover the nature of the ingredients from whence the difference arose. For this purpose, I procured some pieces of each sort of limestone, and first tried what would be their effect upon vegetables, in their natural state, by reducing them to coarse powder, and sowing in them the seeds of different plants. In both kinds, the seeds grew equally well, and nearly in the same manner as they would in sand, or any other substance which affords no nourishment to vegetables. Pieces of each sort of stone were then burnt to lime; and, after they had been exposed for some weeks to the air, that their causticity might be diminished, some seeds were sown in them. In the kind of lime which was found most beneficial to land, almost all the seeds came up, and continued to grow, as long as they were supplied with water; and the roots of the plants had many fibres, which had penetrated to the bottom of the cup in which they grew. Upon examining the composition of this sort of lime, it proved to consist entirely of calcareous earth. By its exposure to the air for about three months, it was found to have absorbed four-fifths of the fixed air required to saturate it. In the other kind, a few only of the seeds grew, and the plants produced from them had hardly any stalks or roots, being formed almost entirely of the two seed-leaves, which lay quite loose upon the surface. This sort of lime, being spread upon a garden soil, to the thickness of about the tenth of an inch, prevented nearly all the seeds which had been sown from coming up, whilst no injury was occasioned by common lime used in the same manner. Upon examining the composition of this substance, which was so destructive to the plants, it was discovered to contain three parts of pure calcareous earth, and two of magnesia. The quantity of fixed air which it had absorbed, by being exposed for about the same time as the pure lime just mentioned, was only 42 hundredths of that combined with it before it was burnt.

As it seemed probable, that the magnesia contained in this lime was the cause of its peculiar properties, the following experiments were made, to determine the effects of that substance upon the growth of vegetables. Some seeds, chiefly of colewort, which were preferred from their growing quickly, were sown in uncalcined magnesia; but, though they sprouted, the leaves never rose above the surface, and the plants were entirely without roots: nor did they appear to grow better in magnesia which had been washed in water containing fixed air. Calcined magnesia was, however, much more destructive, as the seeds would not come up in it. To compare its effects on vegetables with those of lime, each of these earths was mixed, in different proportions, with sand, in small cups, in which, seeds were then sown. The lime was obtained from marble; and, before it was put into the sand, was made to fall to powder, by being moistened with water. In a mixture of four ounces of sand with three or four grains of calcined magnesia, it was a long time before the seeds came up, and the plants had hardly any roots or stalks; and, with ten grains or more of magnesia, there was no appearance of vegetation. Thirty or forty grains of lime did not retard the growth of the seeds more than three or four of magnesia, and the injurious effects were not so lasting. The lime, by absorbing fixed air, soon lost its destructive properties; so that, after keeping these mixtures four or five weeks, seeds were found to grow in that with forty grains of lime, nearly as well as in pure sand; but, in that with four grains of magnesia, they produced only the seed-leaves, as was described before. It was necessary occasionally to break in pieces the sand which had so much lime, as it would otherwise have been too hard to admit the seeds to penetrate through it. Plants will bear a much larger proportion of magnesia in vegetable soil than in sand: with twenty grains, however, of calcined magnesia, in as much soil as was equal in bulk to four ounces of sand, the seeds produced only the seed-leaves, without roots; and, with about forty grains, they were entirely prevented from coming up.

In countries where the magnesian lime is employed, it was said, that the barrenness of any spot on which a heap of it had been laid, would continue for many years. To learn how far it could by time be deprived of its injurious qualities, I procured some pieces of mortar made of this species of lime, from two houses, one of which had been built three, and the other eight years: they were taken from the outside of the building, where they had been exposed to the air. After they were reduced to powder, seeds were sown in them. Only a few came up, and even those produced merely the seed-leaves, without any roots. As plants would grow in the limestone from which this species of lime was formed, although not in the mortar made from it, I wished to know what proportion of the fixed air originally contained in the limestone, had been absorbed by the mortar. For this purpose, a piece of it was finely powdered, to render it of an uniform quality: it was then tried how much of this powder and of the limestone would saturate the same quantity of acid: by this means, I ascertained the proportions of limestone and mortar containing equal quantities of the magnesian lime. The fixed air being obtained from them in those proportions, and measured in an inverted vessel, with quicksilver, it was found, that the mortar which had been exposed three years had absorbed 43, and that of eight years, only 47 hundredths of the quantity originally contained in the limestone. I was not able to obtain any mortar which had been made earlier, though it might deserve to be known how much fixed air it was ultimately capable of absorbing. Common mortar, which had been exposed to the air for a year and three quarters, had regained 63 hundredths of its full quantity of fixed air.

As the preceding experiments were tried during the winter, in a room warmed by fire, perhaps, under circumstances more favourable to vegetation, the same quantity of magnesia would not be equally pernicious.

Magnesian limestone may be easily distinguished from that which is purely calcareous, by the slowness of its solution in acids, which is so considerable, that even the softest kind of the former is much longer in dissolving than marble. From this property of the magnesian limestone, there appeared to be reason for suspecting that the kind of marble which had been called Dolomite, from M. DOLOMIEU, who first remarked its peculiarity in dissolving slowly, might also be similar in its composition. An analysis of this substance was lately given in the *Journal de Physique*, but this is probably erroneous; for, upon examining three specimens, they were found to consist of magnesia and calcareous earth, like the magnesian limestone; so that it ought, no doubt, to be considered as the same species of stone, but in a state of greater purity. The pieces of Dolomite were from different places; one of them being found among the ruins of Rome, where it is thought to have come from Greece, as many statues of Grecian workmanship are made of it, and no quarries of a similar kind are known in Italy; the second was said to have been thrown up by Mount Vesuvius; and the third was from Iona, one of the western islands of Scotland. In many kinds of common marble, small particles and veins may be observed, which are a long time in dissolving. These, upon examination, I discovered to contain a considerable proportion of magnesia; but, as they were probably not quite free from the surrounding marble, I did not ascertain the quantity precisely.

The crystallized structure which may generally be observed in the magnesian limestone, seems to shew that it has not been formed by the accidental union of the two earths, but must have resulted from their chemical combination. The difficulty of dissolving it, may also arise from the attraction of the different component parts to each other. The mortar formed from this kind of lime, is as soluble in acids as common marble; and the substances of which it consists are easily separated. The magnesia may be taken from it by boiling it in muriated lime, and lime is precipitated by it from lime water; but neither of these effects can be produced by the stone, before it is calcined.

Magnesian limestone is probably very abundant in various parts of England. It appears to extend for thirty or forty miles, from a little south-west of Worksop, in Nottinghamshire, to near Ferry-bridge, in Yorkshire. About five or six miles further north there is a quarry of it, near Sherburn; but, whether this is a continuation from the stratum near Ferry-bridge, I have not learnt. From some specimens which were sent me, I find that the cathedral and walls of York are made of it. I have not been able to learn whether there were any shells in the limestone of the tract of country before mentioned. In Mr.

MARSHALL'S account of the agriculture of the midland counties, he speaks of the lime made at Breedon, near Derby, as destructive to vegetables, when used in large quantities. I therefore procured some pieces of it, and they were discovered to contain nearly the same proportion of magnesia as that before described. In this quarry, the stone is frequently crystallized in a rhomboidal form; and petrified shells, not calcareous, but similar in composition to the stone itself, are sometimes, but very rarely, found in it. This substance seems to be common in Northumberland. In the third volume of the Annals of Agriculture, Dr. FENWICK, of Newcastle, observes, that the farmers of that country divide limes into hot and mild. The former of these is no doubt magnesian, as it has similar effects on the soil; and he remarks, that it is not so easily dissolved in acids as the latter. At Matlock, in Derbyshire, the two kinds are contiguous to each other; the rocks on the side of the river where the houses are built being magnesian, and on the other, calcareous. The magnesian rock appears also to be incumbent upon a calcareous stratum; for, in descending a cave formed in this rock, a distinct vein of common limestone may be observed, which contains no magnesia. The latter stratum is very full of shells; but, though there are some also in the magnesian rock, yet they are very rare. In the following tables, containing the analysis of various specimens, some other places are mentioned where this substance is found, but of which I received no further information.

After it was known that the magnesian marble and limestone consisted of the two earths, their proportion was attempted to be discovered, by trying how much gypsum and Epsom salt could be obtained, by means of vitriolic acid, from a certain weight of each specimen. When the superfluous vitriolic acid had been evaporated by heat, the Epsom salt was separated from the gypsum by water. The result of these trials is expressed in the following table.

| | Dry gypsum. | Dry Epsom salt. |
|---|---|---|
| 5 grains of limestone from Breedon gave | 3.9 | 3.15 |
| ——————————— Matlock - | 3.95 | 2.9 |
| ——————————— Worksop - | 3.8 | 3.0 |
| ——————————— York - | 3.8 | 3.1 |
| 3 grains of calcareous spar and 1 grain of calcined magnesia gave - - | 3.9 | 2.7 |

As the preceding method of estimating the quantities of magnesia and calcareous earth is liable to considerable error, I afterwards examined them in the following manner, which seems capable of great exactness. Twenty-five grains of each substance were dissolved by marine acid, in a cup of platina, and, after the solution was evaporated to dryness, it was made red hot for a few minutes. The mass remaining in the cup, which consisted of muriated lime, and of the magnesia freed from the acid, was washed out with water, and poured into a phial. There was then added to it a known quantity of diluted marine acid, somewhat more than was sufficient to redissolve the magnesia, and, after the solution, a certain weight of calcareous spar, part of which would be dissolved by the superfluous acid. By the quantity of spar remaining undissolved, it was learnt how much acid was required to dissolve the magnesia. The iron and argillaceous earth contained in some specimens, were precipitated by the spar, and therefore could not occasion any error. The calcareous spar, however, dissolved more slowly where there was argillaceous earth, as it became coated with it; but this incrustation was occasionally removed, and, in all the experiments, the spar was left in the solution till it suffered no further diminution. For this purpose, it was necessary to keep them slightly warm for some days, during which time, the phials were generally closed, to prevent any escape of the acid.

The first experiment in the following table was made upon known quantities of magnesia and calcareous earth, to try the accuracy of the process. For this purpose, also, the second was repeated upon a piece of limestone, previously powdered, to render every part of it of the same quality. The first column shews the quantity of calcareous spar which might have been dissolved by the acid required to take up the magnesia. The second shews the corresponding quantities of magnesia in 25 grains of each substance. The third expresses the quantity of lime. This was inferred by subtracting the weight of the magnesia, and of the iron and clay, from 13.2 grains, the weight of the whole quantity of earth in 25 grains of limestone. This is probably not very incorrect, as, in two specimens which differed most in the proportion of magnesia and lime, the weight of the two earths was nearly the same.

A piece of Dolomite, from Rome, was wrapped in a thin leaf of platina, that no part of it might be lost, and, being then exposed to a strong heat, left of earth - 52.9 per cent.
Dolomite from Mount Vesuvius - 52.8
Breedon limestone - - - 52.4
Calcareous spar left of lime - - 55.8

In three of the experiments, also, the calcareous earth was precipitated by mineral alkali; and the quantity of it being tried by that of the marine acid required to dissolve it, it corresponded very nearly with that put down.

A quantity of marine acid which would dissolve 15 grains of calcareous spar, would also dissolve 5.5 of calcined magnesia, and

2.5 grains of spar; so that, 12.5 grains of spar required the same quantity of acid as 5.5 grains of magnesia.

The magnesia used was very pure, and made red hot immediately before it was weighed.

| Substances examined. | Quantity of spar which the acid, required to take up the magnesia, would have dissolved. | Quantity of magnesia. | Quantity of lime. | Iron and clay. |
|---|---|---|---|---|
| Mixture of 5.5 grains of magnesia and 14 grains of calcareous spar - - - | 12.5 | 5.5 | 7.8 | 0 |
| 25 grains of Breedon limestone, previously powdered | 11.53 | 5.071 | 7.929 | .2 |
| 25 grains from part of the same powder - - | 11.56 | 5.082 | 7.913 | .2 |
| 25 grains of Dolomite from Rome - - - | 12.2 | 5.37 | 7.73 | .1 |
| ——— Dolomite from Iona - - - - | 10.1 | 4.4 | 7.8 | 1.0 { Insoluble substance. |
| ——— Vesuvian Dolomite - - - - | 10.38 | 4.565 | 8.575 | .06 |
| A second experiment, from part of the same Vesuvian Dolomite - - - | 10.03 | 4.411 | 8.849 | .06 |
| 25 grains of magnesian limestone from Wansworth, near Doncaster - - - | 12.75 | 5.61 | 7.34 | .25 |
| ——— Thorpe arch - | 10.95 | 4.84 | 7.8 | .6 |
| ——— Matlock - | 12.5 | 5.5 | 7.388 | .31 |
| ——— York minster - | 11. | 4.84 | 8.26 | .1 |
| ——— Worksop - | 11.6 | 5.104 | 7.496 | .6 |
| ——— Sherburn - | 11.5 | 5.08 | 7.56 | .56 |
| ——— Westminster-hall | 10.1 | 4.44 | 8.37 | .4. |

# XVIII. *Experiments and Observations on Shell and Bone. By* Charles Hatchett, *Esq. F. R. S.*

Read June 13, 1799.

Some experiments which I lately made at the request of Mr. Home, and which he has done me the honour to mention in his ingenious Paper on the teeth of graminivorous quadrupeds, induced me to turn my attention more particularly to the chemical examination of shell and bone, especially as the former appeared to have been hitherto much neglected.

The time since these experiments were began, has not been sufficient to enable me to enter into all the minutiæ of the chemical analysis of these substances; but, as some remarkable facts were ascertained, I have now ventured to bring them forward, with the addition of some observations, although, as yet, the whole is little more than a very imperfect outline.

The first of these experiments were made on the shells of marine animals; and, to avoid repetition and prolixity, I shall, in a great measure, once for all describe the menstrua, the precipitants, and the mode of operation.

When shells were examined, they were immersed in acetous acid, or nitric acid diluted, according to circumstances, with 4, 5, 6, or more parts of distilled water; and the solution was always made without heat.

The carbonate of lime was precipitated by carbonate of ammoniac, or of potash; and phosphate of lime (if present) was previously precipitated by pure or caustic ammoniac.

If any other phosphate, like that of soda, was suspected, it was discovered by solution of acetite of lead.

Bones and teeth were also subjected to the action of the acetous, or diluted nitric and muriatic acids.

The dissolved portion was examined by the abovementioned precipitants; and, in experiments where the quantity of the substance would permit, the phosphoric acid was also separated by nitric or sulphuric acid. The phosphoric acid thus obtained, was proved, after concentration, by experiments which, being usually employed for such purposes, are too well known to require description.

It is necessary moreover to observe, that as the substances examined were very numerous, and my principal object was to discover the most prominent characters in them, I did not, for the present, attempt in general to ascertain minutely the proportions, so much as the number and quality, of their respective ingredients.

The greater part, if not all, of marine shells, appear to be of two descriptions, in respect to the substance of which they are composed. Those which will be first noticed, have a porcellaneous aspect, with an enamelled surface, and, when broken, are often in a slight degree of a fibrous texture.

The shells of the other division have generally, if not always, a strong epidermis, under which is the shell, principally or intirely composed of the substance called *nacre* or mother of pearl.

Of the porcellaneous shells, various species of Voluta, Cypræa, and others of a similar nature, were examined.

Of the shells composed of *nacre* or mother of pearl, I selected the oyster, the river muscle, the *Haliotis Iris*, and the *Turbo olearius*.

### *Experiments on Porcellaneous Shells.*

Shells of this description, when exposed to a red heat in a crucible, during about a quarter of an hour, crackled and lost the colours of their enamelled surface; they did not emit any apparent smoke, nor any smell like that of burned horn or cartilage. Their figure remained unchanged, excepting a few flaws; and they became of an opaque white, tinged partially with pale gray, but retained part of their original gloss.

The shells which had not been exposed to fire, (whether entire or in powder,) dissolved with great effervescence in the various acids; and the solution afterwards remained colourless and transparent.

But the shells which had been burned, upon being dissolved, deposited a very small quantity of animal coal; and thereby the presence of some gluten was denoted, although the proportion was too small to be discovered in the solution of the shells which had not been burned.

The various solutions were filtrated, and were examined by pure ammoniac and acetite of lead; but I never obtained any trace of phosphate of lime, nor of any other combination of phosphoric acid.

The carbonate of lime was afterwards precipitated by carbonate of ammoniac; and, from many experiments it appeared, that porcellaneous shells consist of carbonate of lime, cemented by a very small portion of animal gluten.

Previous to the experiments on shells composed of nacre or mother of pearl, I examined some Patellæ from Madeira.

When these were exposed to a red heat in a crucible, there was a perceptible smell, like that of horn, hair, or feathers.

The proportion of carbonic matter deposited by the subsequent solution, was more considerable than that of the shells abovementioned; and the proportion of carbonate of lime, relative to their weight, was less.

When the recent shells were immersed in very dilute nitric acid, the epidermis was separated, the whole of the carbonate of lime was dissolved, and a gelatinous substance, nearly liquid, remained; but without retaining the figure of the shell, and without any fibrous appearance.

These shells evidently, therefore, contain a larger portion of a more viscid gelatinous substance than those before mentioned; but the solution, separated from the gelatinous substance, afforded nothing but carbonate of lime.

### *Experiments on Shells composed of Nacre or Mother of Pearl.*

When the shell of the common oyster was exposed to a red heat, the effects were the same as those observed in the Patellæ, and the solution of the unburned shell was similar, only the gelatinous part was rather of a greater consistency.

A species of the river muscle was next subjected to experiment. This, when burned in a crucible, emitted much smoke, with a strong smell of burned cartilage or horn; the shell throughout became of a dark gray, and exfoliated. By solution in the acids, a large quantity of carbonic matter was separated; and much less of carbonate of lime was obtained, from a given weight of the shell, than from those already mentioned.

Upon immersing an unburned shell in dilute nitric acid, a rapid solution and effervescence at first took place, but gradually became less, so that the disengagement of the carbonic acid gas was to be perceived only at intervals.

At the end of two days, I found nearly the whole of the carbonate of lime dissolved; but a series of membranes, retaining the figure of the shell, remained, of which the epidermis constituted the first.

In the beginning, the carbonate of lime was readily dissolved, because the acid menstruum had an easy access; but, after this, it had more difficulty to insinuate itself between the different membranes, and of course the solution of the carbonate of lime was slower.

During the solution, the carbonic acid gas was entangled, and retained in many places between the membranes, so as to give to the whole a cellular appearance.

The *Haliotis Iris*, and the *Turbo olearius*, resembled this muscle, excepting that their membranaceous parts were more compact and dense.

These shells, when deprived by an acid menstruum of their hardening substance, or carbonate of lime, appear to be formed of various membranes, applied stratum super stratum.

Each membrane has a corresponding coat or crust of carbonate of lime; which is so situated, that it is always between every two membranes, beginning with the epidermis, and ending with the last formed internal membrane.

The animals which inhabit these stratified shells, increase their habitation by the addition of a stratum of carbonate of lime, secured by a new membrane; and, as every additional stratum exceeds in extent that which was previously formed, the shell becomes stronger in proportion as it is enlarged;

and the growth and age of the animal become denoted, by the number of the strata which concur to form the shell.

Although the *Haliotis Iris* and the *Turbo olearius* are composed of the true mother of pearl, I was induced to repeat the foregoing experiments, on some detached pieces of mother of pearl, such as are brought from China.

These experiments I need not describe, as the results were precisely the same.

I must, however, observe, that the membranaceous or cartilaginous parts of these shells, as well as of the pieces of mother of pearl, retained the exact figure of the shell, or piece, which had been immersed in the acid menstruum; and these membranaceous parts distinctly appeared to be composed of fibres placed in a parallel direction, corresponding to the configuration of the shell.

The same experiments were made on pearls; which proved to be similar in composition to the mother of pearl; and, so far as their size would enable me to discern, they appeared to be formed by concentric coats of membrane and carbonate of lime; by this structure, they much resemble the globular calcareous concretions, found at Carlsbad and other places, called Pisolithes.

The wavy appearance and irridescency of mother of pearl, and of pearl, are evidently the effect of their lamellated structure and semitransparency; in which, in some degree, they are resembled by the lamellated stone called Adularia.

When the experiments on the porcellaneous shells, and on those formed of mother of pearl are compared, it appears, that the porcellaneous shells are composed of carbonate of lime, cemented by a very small portion of gluten; and that mother of pearl and pearl do not differ from these, except by a smaller proportion of carbonate of lime; which, instead of being simply cemented by animal gluten, is intermixed with, and serves to harden, a membranaceous or cartilaginous substance; and this substance, even when deprived of the carbonate of lime, still retains the figure of the shell.

But, between these extremes there will, probably, be found many gradations; and these we have the greater reason to expect, from the example afforded by the Patellæ, which have been lately mentioned.

Some few experiments were made on certain land shells; and, in the common garden snail I thought that I discovered some traces of phosphate of lime; but, as I did not find any in the *Helix nemoralis*, it may be doubted whether the presence of phosphate of lime should be considered as a chemical character of land shells.*

## *Experiments on the covering Substance of crustaceous Marine Animals.*†

As I was not acquainted with any experiments by which the chemical nature of the substance which covers crustaceous marine animals had been determined, I was desirous to ascertain in what respect it was different from shell, and I began these experiments on three species of the Echinus, with which I had been favoured by the Right Honourable President.

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* Some experiments which I have lately made upon the cuttle-bone of the shops, have proved, that the term bone is here misapplied, if the presence of phosphate of lime is to be regarded as the characteristic of bone; for, this substance, in composition, is exactly similar to shell, and consists of various membranes hardened by carbonate of lime, without the smallest mixture of phosphate.

† Under this head I have included my experiments upon Echini, Star-fish, Crabs, Lobsters, &c.

I was the more inclined to begin with the Echini, because naturalists do not appear to be perfectly agreed, whether to call them testaceous or crustaceous animals.

KLEIN, who has written a work upon Echini, after having noticed the various opinions of RONDELET, RUMPHIUS, and others, determines that they are to be regarded as testaceous animals. His words are, " Sic plurimas testas marinas, in " statu naturali consideratas, cum echinodermatis potius quam " cum crustis astacorum vel cancrorum conferre licebit. Itaque " echinoderma, cum ARISTOTELE, qui echinos inter testacea quibus facultas ingrediendi est reponit, nec non cum BELONIO, " ALDROVANDO, et excellentissimo SLOANIO, religiosé testam " appellamus, quam satis duram in nonnullis offendimus."*

But LINNÆUS was of the contrary opinion, as appears from his definition of the echinus. " Corpus subrotundum, *crusta* " *ossea tectum*, spinis mobilibus sæpius aspera."†

Now, as the experiments above related had proved, that the shells of marine animals were composed of carbonate of lime, without any phosphate, I thought it very possible, that the covering of the crustaceous animals might, in some respect, be different, and if so, I should thus, by chemical characters, be enabled to ascertain the class to which the Echinus was to be referred.

Of the three Echini which were examined, one had small spines; the second had large obtuse spires; and the third was of a very flat form.

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* KLEIN, *Naturalis dispositio Echinodermatum, &c.* p. 10.

† *Systema Naturæ.* Edit. GMELIN, p. 3168.

Portions of these echini were separately immersed in acetous, muriatic, and diluted nitric acid, by each of which they were completely dissolved, with much effervescence; depositing, at the same time, a thin outer skin or epidermis. The transparency of the solutions was also disturbed by a portion of gluten, which remained suspended, and communicated a brownish colour to the liquors.

The solutions in acetous and diluted nitric acid were filtrated; after which, from the acetous solution of each Echinus, I obtained a precipitate of phosphate of lead, by the addition of acetite of lead; and, having thus proved the presence of phosphoric acid, I saturated the nitric solutions with pure ammoniac, by which a quantity of phosphate of lime was obtained, much inferior, however, in quantity, to the carbonate of lime, which was afterwards precipitated by carbonate of ammoniac.

The composition of the crust of the Echinus is therefore different from that of marine shells; and, by the relative proportions and nature of the ingredients, it approaches most nearly to the shells of the eggs of birds; which, in like manner, consist of carbonate, with a small proportion of phosphate of lime, cemented by gluten.

It remained now, to examine the composition of those substances which are decidedly called crustaceous; but, previous to this, some experiments were made on the Asterias or star-fish, of which I took the species commonly found on our coasts, and known by the popular name of five fingers, (Asterias rubens.)

The Asterias is thus described by LINNÆUS. "Corpus de-"pressum, subtus sulcatum: crusta coriacea, tentaculis muri-"cata."*

When the Asterias was immersed in the acids, a considerable effervescence was produced, and a thin external stratum was dissolved; after which, it remained in a perfectly coriaceous state, and complete, in respect to the original figure.

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* *Systema Naturæ.* Edit. Gmelin. p. 3160.

The dissolved portion, being examined by the usual precipitants, proved to be carbonate of lime, without any mixture of phosphate; but, in another species of the Asterias, which had twelve rays, *(Asterias papposa,)* I discovered a small quantity of phosphate of lime. I am therefore induced to suspect, that in the different species of the Asterias, nature makes an imperfect attempt to form shell on some, and a crustaceous coating on others; and that a series of gradations is thus formed, between the testaceous, the crustaceous, and the coriaceous marine animals.

It was now requisite to ascertain if phosphate of lime is a component part of the substance which covers the crustaceous marine or aquatic animals, such as the crab, lobster, prawn, and crayfish.

Pieces of this substance, taken from various parts of those animals, was, at different times, immersed in acetous, and in diluted nitric acid; those which had been placed in the diluted nitric acid, produced a moderate effervescence, and in a short time were found to be soft and elastic, of a yellowish-white colour, and like a cartilage which retained the original figure.

The same effects were produced by acetous acid, but in a less degree; in the latter case also, the colouring matter remained, and was soluble in alcohol.

All the solutions, both acetous and nitric, afforded carbonate and phosphate of lime, although the former was in the largest proportion.

There is reason to conclude, therefore, that phosphate of lime, mingled with the carbonate, is a chemical characteristic which distinguishes the crustaceous from the testaceous substances; and that the principal difference in the qualities of each, when complete, is caused by the proportion of the hardening substances, relative to the gluten by which they are cemented; or by the abundance and consistency of the gelatinous, membranaceous, or cartilaginous substance, in and on which, the carbonate of lime, or the mixture of carbonate and phosphate of lime, has been secreted and deposited. Moreover, as the presence of phosphate of lime, mingled with carbonate, appears to be a chemical character of crustaceous marine animals, there is every reason to conclude that LINNÆUS did right not to place the Echini among the testaceous ones.

The presence of phosphate of lime, in the substance which covers the crustaceous marine animals, appears to denote an approximation to the nature of bone, which, not only by the experiments of Mr. GAHN, but by the united testimony of all chemists, has been proved principally to consist (as far as the ossifying substance is concerned) of phosphate of lime.

This consideration, therefore, induced me to repeat the above experiments, on the bones of various animals.

It is scarcely necessary for me to mention the usual effects of acids on bones steeped in them, as they are known to every physiologist and anatomist.

In every operation of this nature, the ossifying substance, which is principally phosphate of lime, is dissolved, and a cartilage or membrane, of the figure of the original bone, remains; so that the first origin of bones appears to be by the formation of a membrane or cartilage, of the requisite figure, which, when the subsequent secretion of the ossifying substance takes place, is penetrated by it, and thus becomes more or less converted into the state of bone.

It is also known, that the nature of the bone is more influenced by the greater or less predominance of the membranaceous or cartilaginous part, than by any other cause. It is not, therefore, for me to add any thing to this part; and, in respect to the substance which is the cause of ossification, little also requires to be mentioned, for this (as has been already observed) is known principally to consist of phosphate of lime. I shall only, therefore, briefly mention the results of certain experiments.

The bones of fish, such as those of the salmon, mackerel, brill, and skate, afforded phosphate of lime; and the only difference was, that the bones of these fish appeared in general to contain more of the cartilaginous substance, relative to the phosphate of lime, than is commonly found in the bones of quadrupeds, *&c.*

The different bones, also, of the same fish, were various in this respect; and the bones about the head of the skate, only differed from cartilage, by containing a moderate proportion of phosphate of lime.

It is at present believed that phosphate, with some sulphate of lime, constitutes the whole of the ossifying substance; and perhaps the formation of bone from cartilage, depends only on the phosphate of lime; but, whether this is the case or not, it is fit that I should notice a third substance, which constantly occurred in the course of my experiments.

When human bones, or teeth, as well as those of quadrupeds and fish, whether recent or calcined, were exposed to the action of acids, an effervescence, although at times but feeble, was produced. This circumstance, at first, I did not particularly notice, but the following experiments excited my attention.

After the phosphate of lime had been precipitated from the solutions of various teeth and bones, by pure ammoniac, I observed, that a second precipitate, much smaller in quantity, was obtained by the addition of carbonate of ammoniac. This second precipitate dissolved in acids, with much effervescence, during which, carbonic acid was disengaged; and selenite was formed by adding sulphuric acid. Moreover, the solution of this precipitate did not contain any phosphoric acid; nor did the liquor from which the precipitate had been separated afford any trace of it.

This precipitate was therefore carbonate of lime; but I still was not certain that it existed, as such, in the teeth and bones.

Although regular and comparative analyses of the bones of different animals have not hitherto been made, yet, by the experiments of Messrs. GAHN, SCHEELE, MACQUER, FOURCROY, BERNIARD, and the Marquis de BULLION, it has been proved, that phosphate of lime is the principal ossifying substance of bones in general, and that this is accompanied by a small proportion of some saline substances, and by sulphate of lime.

I was therefore desirous to ascertain, whether the carbonate of lime which I had obtained by the abovementioned experiments, had been produced from the sulphate of lime decomposed by the alkaline precipitant, or whether the greater part had not existed in the bones, in the state of carbonate.

Each of the solutions in nitric acid afforded a precipitate with nitrate of barytes; but the quantity of sulphuric acid thus separated, appeared by far too small to be capable of saturating the whole of the carbonate of lime obtained from an equal quantity of the solution. To prove, therefore, the presence of the carbonic acid, and the consequent formation of carbonate of lime, portions of the various teeth and bones were immersed, at separate times, in muriatic acid; and the gas produced was received in lime water, by which it was speedily absorbed, and a proportionate quantity of carbonate of lime was obtained.

Although it appears, that the principal effects during ossification are produced by phosphate of lime, yet we here see, that not only some sulphate, but also some carbonate of lime, enters the composition of bones; and it is not a little curious to observe, that as the carbonate of lime exceeds in quantity the phosphate of lime in crustaceous marine animals, and in the egg shells of birds, so in bones it is *vice versa.* It is possible, when many accurate comparative analyses of bones have been made, that some may be found composed only of phosphate of lime; and that thus, shells containing only carbonate of lime, and bones containing only phosphate of lime, will form the two extremities of the chain.

I shall now make a few remarks on the enamel of teeth.

When a tooth coated with enamel is immersed in diluted nitric or muriatic acid, a feeble effervescence takes place, and the enamel is completely dissolved; so also is the bony part, but the cartilage of that part is left, retaining the shape of the tooth. Or, if a tooth in which the enamel is intermixed with the bony substance, is plunged in the acid, the enamel and the bony part are dissolved, in the same manner as before; that is to say, the enamel is completely taken up by the acid, while the tooth, like other bones, remains in a pulpy or cartilaginous state, having been deprived of the ossifying substance. Consequently, those parts which were coated or penetrated by lines of enamel, are diminished, in proportion to the thickness of the enamel which has been thus dissolved; but little or no diminution is observed in the tooth.*

Mr. Hunter has noticed this; and, speaking of enamel, says, "when soaked in a gentle acid, there appears no gristly or "fleshy part with which the earthy part had been incor-"porated."†

Now, when the difference which has been lately stated, between porcellaneous shell and mother of pearl, is considered, it is not possible to avoid the comparing of these to enamel and tooth.

When porcellaneous shell, whole or in powder, is exposed to the action of acids, it is completely dissolved, without leaving any residuum.

Enamel is also completely dissolved, in the like manner.

Porcellaneous shell and enamel, when burned, emit little or no smoke, nor scarcely any smell of burned horn, or cartilage.

Their figure, after having been exposed to fire, is not materially changed, except by cracking in some parts: their external gloss partly remains, and their colour at most becomes gray, very different from what happens to mother of pearl, or tooth. In their fracture they have a fibrous texture; and, in short, the only essential difference between them appears to be, that porcellaneous shell consists of carbonate of lime, and enamel of phosphate of lime, each being cemented by a small portion of gluten.

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* I have also observed, that when raspings of enamel are put into diluted nitric or muriatic acid, they are dissolved without any apparent residuum; but, when raspings of tooth or bone are thus treated, portions of membrane or cartilage remain, corresponding to the size of the raspings.

† Natural History of the Human Teeth, page 35.

In like manner, if the effects produced by fire and acid menstrua, on shells composed of mother of pearl, and on the substance of teeth and bone, are compared, a great similarity will be found; for, when exposed to a red heat,

1st. They smoke much, and emit a smell of burned cartilage, or horn.

2dly. They become of a dark gray or black colour.

3dly. The animal coal thus formed is of difficult incineration.

4thly. They retain much of their original figure; but the membranaceous shells are subject to exfoliate.*

5thly. These substances, (pearl, mother of pearl, tooth, and bone,) when immersed in certain acids, part with their hardening or ossifying substances, and then remain in the state of membrane or cartilage.

6thly. When previously burned, and afterwards dissolved in acids, a quantity of animal coal is separated, according to the proportion of the gelatinous, membranaceous, or cartilaginous substance, and according to the duration of the red heat.

And lastly, the acid solutions of these substances, by proper precipitants, afford carbonate of lime, in the one case, and phosphate of lime principally, in the other, in a proportion relative to the membrane or cartilage with which, or on which, the one or the other had been mixed, or deposited.

As porcellaneous shell principally differs from mother of pearl, only by a relative proportion between the carbonate of lime and the gluten or membrane, in like manner, the enamel appears only to be different from tooth or bone, by being destitute of cartilage, and by being principally formed of phosphate of lime, cemented by gluten.

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* This is a natural consequence, arising from their structure.

The difference, in the latter case, seems to explain why the bones and teeth of animals fed on madder become red, when, at the same time, the like colour is not communicated to the enamel; for it appears probable, that the cartilages which form the original structure of the teeth and bones, become the channels by which the tinging principle is communicated and diffused.

These comparative experiments prove, that there is a great approximation in the nature of porcellaneous shell and the enamel of teeth, and also in that of mother of pearl and bone; and, if a shell should be found composed of mother of pearl coated by the porcellaneous substance, it will resemble a tooth coated by the enamel, with the difference of carbonate being substituted for phosphate of lime.

Some experiments on cartilaginous substances (which I intended to have inserted in this Paper, but which I am prevented from doing, as they are not as yet sufficiently advanced,) have in a great measure convinced me, that membranes and cartilages (whether destined to become bones by a natural process, as in young animals, or whether they become such by morbid ossification, as often happens in those which are aged,) do not contain the ossifying substance, or phosphate of lime, as a constituent principle. I mean by this, that I believe the portion of phosphate of lime found in cartilaginous and horny substances to be simply mixed as an extraneous matter; and that, when it is absent, membrane, cartilage, and horn, are most perfect and complete.

The frequent presence of phosphate of lime in cartilaginous substances, is not a proof of its being one of their constituent principles, but only that it has become deposited and mixed with them, in proportion to the tendency they may have to form modifications of bone; or according to their vicinity with such membranes or cartilages as are liable to such a change. If horns are examined, few I believe will be found to contain phosphate of lime in such a proportion as to be considered an essential ingredient. I would not be understood to speak here of such as stag or buck horn, for that has every chemical character of bone, with some excess of cartilage; but I allude to those in which the substance of the horn is distinctly separate from the bone, and which, like a sheath, covers a bony protuberance which issues from the os frontis of certain animals.*

Horns of this nature, such as those of the ox, the ram, and the chamois, also tortoise shell, afford, after distillation and incineration, so very small a residuum, of which only a small part is phosphate of lime, that this latter can scarcely be regarded as a necessary ingredient.

By some experiments made on 500 grains of the horn of the ox, I obtained, after a long continued heat, only 1,50 gr. of residuum; and, of this, less than half proved to be phosphate of lime.

78 grains of the horn of the chamois afforded only 0,50 of residuum; and 500 grains of tortoise shell yielded not more than 0,25 of a grain, of which, less than half was phosphate of lime.

Now it must be evident, that so very small a quantity cannot influence the nature of the substances which afforded it; and the same may be said of synovia, 480 grains of which did not yield more than one grain of phosphate of lime.

This substance is undoubtedly various in its proportions, in all these and other animal substances, arising probably from the age and habit of the animal which has produced them; but I believe that I may, at least, venture to place some confidence in the foregoing experiments, as several others, made since the above was written, have tended to confirm them.*

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* Nature seems here to have made an analysis or separation of horn from bone.

In the course of making the experiments which have been related, I examined the fossil bones of Gibraltar, as well as some glossopetræ or shark's teeth. The latter afforded phosphate and carbonate of lime; but the carbonate of lime was visibly owing principally to the matter of the calcareous strata which had inclosed these teeth, and which had insinuated itself into the cavities left by the decomposition of the original cartilaginous substance.

The bones of the Gibraltar rock also consist principally of phosphate of lime; and the cavities have been partly filled by the carbonate of lime which cements them together.

Fossil bones resemble bones which, by combustion, have been deprived of their cartilaginous part; for, they retain the figure of the original bone, without being bone in reality, as one of the most essential parts has been taken away. Now, such fossil or burned bones can no more be regarded as bone, than charcoal can be considered as the vegetable of which it retains the figure and fibrous structure.

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* These experiments were repeated on bladder, which I chose in preference to any other membrane, as not being liable to ossification, and therefore likely to contain very little or no phosphate of lime.

250 grains of dry hogs' bladder, after incineration, left a residuum the weight of which did not exceed $\frac{1}{50}$th of a grain. This was dissolved in diluted nitric acid; and, upon adding pure ammoniac, some faint traces of phosphate of lime were observed. Now, as 250 grains of bladder did not afford more than $\frac{1}{50}$th of a grain of residuum, of which only a part consisted of phosphate of lime, there is much reason to regard this experiment as an additional proof, that phosphate of lime is not an essential ingredient of membrane.

Bones which keep their figure after combustion, resemble charcoal made from vegetables replete with fibre; and cartilaginous bones which lose their shape by the same cause, may be compared to succulent plants which are reduced in bulk and shape in a similar manner.

From these last experiments, I much question if bodies consisting of phosphate of lime, like bones, have concurred materially to form strata of limestone or chalk; for, it appears to be improbable that phosphate is converted into carbonate of lime, after these bodies have become extraneous fossils.

The destruction or decomposition of the cartilaginous parts of teeth and bones in a fossil state, must have been the work of a very long period of time, unless accelerated by the action of some mineral principle; for, after having, in the usual manner, steeped in muriatic acid the os humeri of a man brought from Hythe, in Kent, and said to have been taken from a Saxon tomb, I found the remaining cartilage nearly as complete as that of a recent bone. The difficult destructibility of substances of a somewhat similar nature, appears also from the mining implements formed of horn, which are not unfrequently found in excavations of high antiquity.

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*Continued from the Philosophical Transactions for the Year* 1798, *Page* 593.

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*From the Press of*
*W. BULMER & Co.*
*Cleveland-Row, St. James's.*

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